

THURSDAY, SEPTEMBER 1, 1887.

HIGHER ALGEBRA.

Higher Algebra: a Sequel to Elementary Algebra for Schools. By H. S. Hall, M.A., and S. R. Knight, B.A. (London: Macmillan, 1887.)

ANYONE who imagined from the shortened title of this volume, that the work extended to that higher region of algebra to which Salmon's well-known "Lessons" are "introductory," would be surprised to find that it contains little beyond what may fairly be regarded as "elementary algebra." Indeed it appears to us that much that is contained in the earlier chapters would have found its place more appropriately in the same authors' "Elementary Algebra for Schools," using their own device of an asterisk to indicate those articles which might, in the case of the ordinary school-boy, be omitted or reserved for a second reading; and thus the awkwardness of breaking up such subjects as ratio, proportion, and progressions into separate parts, by an arbitrary division into lower and higher, would have been avoided.

Apart from this defect of plan, the work before us has great merits as a text-book adapted to the wants of the ordinary student of algebra and to the exigencies of examinations. It is a development and improvement upon "Todhunter," as "Todhunter" was a development and improvement upon "Wood." The main framework is the same: many of the proofs of algebraical theorems have been replaced by better proofs, and new matter has been introduced. Still it remains essentially an artificial framework and has no claim to be regarded as an organic growth from a few central principles, with a corresponding natural relation and affiliation of parts. Thus we find the fundamental laws of algebra for the first time gathered together and discussed in the thirty-fourth chapter (p. 429) of this volume, a chapter of "Miscellaneous Theorems and Examples" for which apparently no fitting place could be found in the framework. It also includes such a fundamental theorem as that known as the "remainder theorem"—that $f(a)$ is the remainder when the rational integral function $f(x)$ is divided by $x - a$ —some of its applications, as well as some discussion of symmetrical expressions and identities.

An elementary algebra, written by a master of modern algebraical science in the light of the higher views of the essential nature of algebra which modern investigations have established, and yet with such simplicity that it may be put into the hands of the school-boy, is a desideratum the advent of which is perhaps foreshadowed, though not fully realized, in respect of simplicity at any rate, in Prof. Chrystal's recent work. It would be obviously unfair to criticize the present work from this point of view: our remaining remarks on it, therefore, will be confined to some matters of detail in the order of the chapters of the book itself.

Perhaps the strongest part of the book is the examples, both those which are worked out, and those which are added to each chapter as exercises for the pupil. As far as we have been able to examine them,

they are sufficiently numerous, well chosen, and instructive, and also well arranged in each exercise in the order of their difficulty. We are surprised to find in the chapter on "Miscellaneous Equations" that there is no hint or caution given that the root obtained may not satisfy the original equation unless the sign of one or more of the radicals involved in it is changed. In fact, in the example worked out on p. 99, the root $x = 6a$ gives by substitution in the equation $2a - 6a = 4a$! We hold that in all such cases the student should be required to show with what signs of the radicals in the equation each solution is consistent, and what combinations of their signs are impossible; otherwise more than half the instructiveness of the example is lost.

The chapters on ratio and proportion need no remark; but that on variation, as in most books of algebra, is in our opinion unsatisfactory, from the fact that the attention of the student is not called to the distinction between a magnitude and its numerical measure. If A stand for the distance and B for the time, when the speed is uniform, "A varies as B" is a statement true of the magnitudes themselves independent of any particular mode of measuring them; but when from this is deduced the equation $A = mB$, either A and B must be regarded as numerical measures of the distance and time with reference to some particular units, in which case m will have a value depending on the units selected; or else m is a multiplier which, besides altering the numerical value of B into that of A, converts a time into a distance, an extension of the notion of multiplication which, if admitted, ought to be very carefully noted and explained.

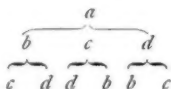
After chapters on progressions, we come to one on scales of notation, though there is no reason, apart from the traditional place it has occupied in books on algebra, why such simple questions as are discussed in it, which, if arithmetic were rationally taught, would have been treated in connexion with the theory of decimal numeration and notation, should be regarded as forming a chapter of "Higher Algebra." The algebraical formulæ which encumber this chapter should only be introduced as summing up what has been previously proved in particular instances by direct reasoning from first principles, not in order to prove the propositions themselves.

It would have been well if the chapter on the theory of quadratic equations had been made one on that of quadratic expressions. By not doing this the opportunity is lost of exemplifying the notion of continuity in the changes of such expressions with the change of the variable both in magnitude and sign and their maximum and minimum values, as well as the introduction of the graph (as Prof. Chrystal has done), to illustrate these changes.

The authors state in their preface that the part of algebra which is concerned with permutations and combinations "is made far more intelligible to the beginner by a system of common-sense reasoning from first principles than by the proofs usually found in algebraical text-books," a proposition with which we heartily agree, only that we see no reason why it should be confined to this particular part of algebra.

When we turn, however, to the chapter on permutations and combinations, except that there is a greater variety of proofs, we fail to find any further appeal to

"common-sense reasoning from first principles" than in other text-books. In fact, in some of the proofs the crucial point of the proposition, instead of being elaborated, is so condensed as to make it very difficult to understand, though it is certainly put in a form which may be easily carried into an examination to the perplexity of the examiner, who may well be in doubt whether the examinee who reproduces the words really sees the point of the proof. We hold that the true way of appealing to "common sense" is to take particular cases first, and when these are grasped, the general proof becomes easy. Thus, to find the number of permutations of 4 things (a, b, c, d) taken 3 together, it is plain that the arrangement



repeated for each of the 4 letters in the top line will give all possible permutations, and that the number is therefore $4 \times 3 \times 2$, and further that the principle of arrangement may be extended to any number of things. This is the essence of the proof given on page 116. It may be said that such exemplification is in the province of the teacher rather than in that of the text-book, but we fear there are many teachers who fail to make things clear in this way to their pupils.

The proof, or rather proofs, of the binomial theorem for positive integral indices are distinct improvements on the cumbrous proof given in Todhunter, the theorem being shown, as it should be, to be a direct consequence of the multiplication of n binomial factors. Euler's proof for any index is carefully stated, and its crucial point emphasized by a preliminary discussion.

Following the binomial theorem comes a chapter on logarithms, which in our opinion would have better followed the chapter on indices in the "Elementary Algebra," as that on interest and annuities might have followed those on progressions. The exponential and logarithmic series would then have followed naturally as a development of the binomial theorem.

The authors have given a chapter on the convergency and divergency of series, in which this important subject is treated with unusual care. We may perhaps demur to the sweeping character of the statement (p. 249) that "the use of divergent series in *mathematical reasoning* leads to erroneous results," but the student cannot be too early or too emphatically warned that a result obtained by the use of divergent series should be verified by other means.

The chapters which follow treat of intermediate coefficients, partial fractions, recurring series, continued fractions, indeterminate equations of the first degree, recurring continued fractions, and indeterminate equations of the second degree, summation of series, the usual elementary theorems of the theory of numbers, the general theory of continued fractions, and probability. All these subjects appear to us to be judiciously and adequately treated, though we should have been glad to see a little more of "common-sense reasoning from first principles" in the elementary chapter on continued fractions, by which it might easily and with advantage have been made to take its place among the chapters of the "Ele-

mentary Algebra." In the chapter on summation of series, the authors, as they tell us in the preface, have laid much stress on the "method of differences." As they have gone so far, we think it is a pity that they have not introduced the notation and the elementary propositions of the calculus of differences, which seem to us very naturally to fall within the limits of algebra. In any case, in their use of the symbol Σ they should not have deviated from its proper meaning by making Σn , for instance, include the term n instead of denoting by it the series ending with $n-1$.

Here the ordinary treatises on algebra end. Our authors have, however, very wisely added a chapter on determinants, containing a satisfactory and sufficient discussion of determinants of the second and third orders, with a useful series of examples of their application, and an indication of the general properties of determinants of any order. The study of this chapter will enable the student to read, without difficulty, treatises on analytical geometry, and afford a good introduction to special works on determinants in general.

Following this is the chapter on miscellaneous theorems and examples, of which we have before spoken, containing a short discussion of the fundamental laws of algebra, then the "remainder theorem," and synthetic division, symmetrical and alternating functions, and elimination.

While the end of ordinary algebra and its various direct applications is undoubtedly a suitable place for a re-discussion of its fundamental laws, as preliminary to the interpretations of double algebra and to the various higher algebras with different fundamental laws, it is strange that our authors have not found the desirability, indeed the necessity, of introducing the other subjects of this chapter, with the exception perhaps of elimination, at a much earlier stage, and as part of a regular sequence in the development of algebraic operations.

The book concludes with a chapter containing the elementary parts of the theory of equations—on the whole judiciously selected. We note it, however, as a defect in this, as in all other treatises we have met with, that Horner's process for approximating to the roots of numerical equations is barely mentioned. We hold that the simplicity and generality of this process is such that it ought to be taught, as a rule (without proof), for finding the roots of numbers, in all treatises on arithmetic, to the exclusion of the cumbrous, unconstructive, and utterly useless method of finding cube roots only, which is usually given; while the proof of the process, which may be made quite easy and intelligible, and its general application to numerical equations, ought to occupy a prominent and early position in every treatise on algebra. Every one who has made himself expert in the use of Horner's method will, we are sure, agree with us that it gives a power in discussing an algebraical expression with numerical coefficients, which can be obtained in no other way.

R. B. H.

OUR BOOK SHELF.

Outlines of Quantitative Analysis. By A. H. Sexton. (Charles Griffin and Co., 1887.)

It is perhaps as great an evil to err on the side of trying too much as to do too little where more might be done. In this book, intended, as the author tells us, to be put

into the hands of students who have but little time to spare and may not intend to become professional chemists; a very wide analytical field is got over; indeed a little too much is attempted in the space, and sacrifices have in nearly all cases to be made where "shortness and simplicity" is the combined ruling idea.

We fully agree with what the author says as to the educational value of quantitative analysis. It is indeed high time that our more elementary students should have the long courses of qualitative analysis shortened, and some more *exact* exercises substituted.

In the course of the 127 pages of this book, including six for tables, we are introduced to the balance, and it is much to be regretted that more has not been said about it. What is said is purely practical—how to turn up the handle and put on the weights.

The first exercises are the determination of water in a carbonate and the ash in several substances, after which a couple of specific gravity methods are given, and then we pass to "simple gravimetric analysis," iron, silver, barium, lead, &c. In the silver exercise the factor 0.75276 is introduced to get the actual silver from the weight of chloride found, and this "factor" is given in all other analyses. It is not of much use any way, and for beginners it is not advisable, as it binds them down to the book, and no appreciable time is saved for ordinary analysis calculations.

The directions for volumetric analysis are very good, and the exercises are well arranged in order of difficulty. The separation exercises and miscellaneous examples will need some attention from the teacher.

In the description of organic analysis—combustion of carbon compounds—the closed-tube process is well described, and a student might be able to do a combustion from the description only; but we are not informed, when the open tube is spoken of, whether the same length, viz. 18 inches, will be sufficient or not. By inference it will. We venture to say that a very doubtful analysis, especially of a volatile body, would result from the use of an open tube only 18 inches long. The description here is much too slight to work by.

The tables at the end are sensible—only just those wanted in the course of the work in the book itself.

Qualitative Chemical Analysis. By Dr. C. Remigius Fresenius. Tenth Edition. Translated and edited by Charles E. Groves, F.R.S. (London: J. and A. Churchill, 1887.)

THE fifteenth German edition of this well-known book contains many emendations and additions, especially in the concluding portions devoted to the reactions of the alkaloids and the systematic methods of detecting them. Of this edition of the original work the present edition of the English translation is as nearly as possible an exact reproduction, and much credit is due to the translator and editor for the care with which he has accomplished a very difficult task. Various styles of type and other typographical improvements have been introduced, in the hope, as Mr. Groves explains, that the book may thereby be rendered more handy and useful to students.

Melting and Boiling Point Tables. Vol. II. By Thomas Carnelley, D.Sc., and Professor of Chemistry in University College, Dundee. (Harrison and Sons, 1887.)

THE issue of vol. ii. of this important work completes it. It is not too much to say that these two volumes will be found in every laboratory. Their compilation represents an amount of patient work from which most men would have recoiled; and the total result, which has cost ten years of effort, reflects the highest credit upon Prof. Carnelley.

Part II., dealing with organic compounds, brings the data down to 1885.

Part III. deals with vapour tensions and boiling points of simple substances, and freezing and melting points of cryohydrates, including facts recorded in 1886.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Law of Error.

EVERYONE interested in the theory of statistics is aware how strongly Quetelet was under the conviction that there is only one law of error (or curve of facility, to use the corresponding expression for the graphical representation of the law) prevalent for the departure from the mean of a number of magnitudes or measurements of any natural phenomenon. I have done what I can to protest against this doctrine as a theoretic assumption; and recently Mr. F. Galton and Mr. F. Y. Edgeworth have shown in some very interesting and valuable papers in the *Philosophical Magazine* and elsewhere how untenable it is, and how great is the importance of studying the properties of other laws of error than the symmetrical binomial, and its limiting form the exponential.

I have been making some calculations recently, principally in the field of meteorology, and I should be extremely glad of the

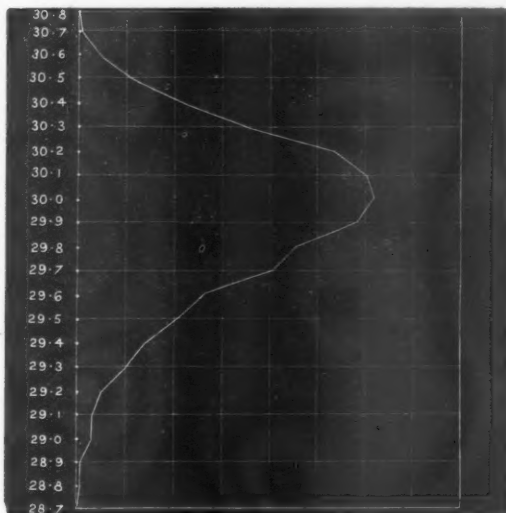


FIG. 1.

judgment and criticism of any of your readers who may be better versed in this science than myself. It must be carefully understood that the questions here raised are solely these:—(1) Do the magnitudes, when arranged in order of their departure from the mean, display a *symmetrical* arrangement? (2) If so, is this arrangement in accordance with the binomial or exponential law?

The first diagram represents the grouping, in respect of relative frequency, of 4857 successive barometric heights. They are from the observations of Mr. W. E. Pain, of Cambridge, and show the readings at 9 a.m. on successive mornings for about thirteen years from January 1, 1865. They are the results of the same instrument, which has required no correction or alteration during that period. They are given to the first decimal place.

The second diagram refers to a similar set of 4380 thermometric observations (1) of the maximum, (2) of the minimum temperature on successive days¹ from January 1, 1873.

In regard to the first diagram the asymmetry is obvious. I have tested the conclusion in the usual way. For instance, the total of 4857 observations was composed of seven batches of a little less than two years each. Precisely the same asymmetry, in varying degrees, is displayed by each of these batches. The asymmetry is of course obvious to the eye in the diagram, but various numerical tests may be proposed. For instance, we may compare (1) the position of the mean value (in this case 29.91) between the extreme values, (2) the relative positions of the maximum ordinate and the mean ordinate, (3) the comparative magnitudes of the "mean errors" to the right and the left of the mean ordinate. They all yield a result in the same direction.

I should be very glad if any of your readers could confirm (or correct) these results by those of more extended observations, or by results taken from other districts. That something of this kind should be displayed where, as here, we are dealing with a one-ended phenomenon—i.e. with one in which unlimited variation was conceivable in one direction but not in the other—seems to me in itself reasonable. But I was certainly surprised to find it so marked, considering how small is the fluctuation in relation to the actual magnitude of the variable phenomenon.

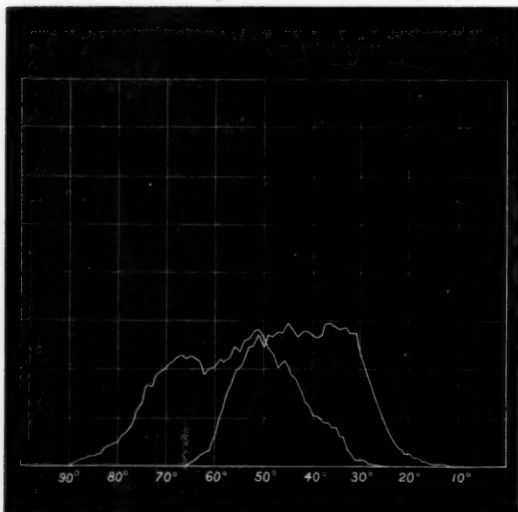


FIG. 2.

It seems to suggest that the common theoretic assumption of a sort of fixed mean or type which is swayed about by a large number of equal and opposite independent disturbing causes, does not hold good in this case.

As regards the second diagram, the two curves are (especially that of the minima) tolerably symmetrical, but they depart widely from anything approaching to Quetelet's supposed fixed type.

Anyone looking at the curve of maxima would say at once that it mingled the results of two distinct means (in Quetelet's phrase), as if we were to group together the observed statures of a great many Scotchmen and Frenchmen. That we are mingling results of distinct means seems true enough, but not of two such, and I cannot account for the two peaks in the curve. What I should have expected would have been something of this kind: Each day has its own appropriate mean maximum (subject to the usual fluctuation), and these mean maxima are themselves grouped about their mean, hence the true mean of all ought to be decidedly the commonest result, i.e. the curve should have a single vertex.

The facts are quite otherwise. The depression towards the

¹ In this case, as the lengths of the successive ordinates from the original data were very irregular, I have smoothed the curve out by taking the mean of three successive heights. For instance, to take the actual figures, the number of occasions on which the maxima were 58°, 59°, and 60°, were respectively 108, 99, and 124; I have assigned the number 110 to 59°, and so on.

centre is far too deep to be accidental, and the final mean (i.e. about 57°) is very far from being the commonest value.

Somewhat similar remarks may be made about the curve of minima. There is some evidence (though not conclusive) of a depression towards the centre in this case also, and the curve is very fairly symmetrical. But the true mean of all the minima cannot claim any numerical preponderance over any other value between 32° and 52°.

I am far too deeply conscious of the numerous pitfalls which lurk about the statistician's path to offer these results with any great confidence. But considering how large is the number of observations included, it certainly seems to me that they call for some explanation. There may of course be some blunder in the calculations, but I have done my best to guard against this. What I trust is that these results may be the means of calling forth some discussion by practised experts in this branch of statistical inquiry, which may serve to confirm or correct my results, and in the former case to offer some explanation of the causes of the phenomena. Very likely this practical inquiry has been already undertaken elsewhere, but the statistics of meteorology are so vastly extensive that it is impossible for any but a professional student of the subject to be acquainted with what goes on in it.

Cambridge.

J. VENN.

The Sense of Smell in Dogs.

WILL Mr. Russell (whose letter in NATURE of August 4 I have just read) be so good as to make another experiment with his pug bitch? He says that she had been "taught to hunt" for biscuit; probably she was also enjoined to "find it," or something similar, when she came into the room. Can he manage to try her powers without awakening her expectation?

I ask it because it seems to me that in this case (and many others) we have something different to observe than mere quickness or keenness of sense, and something well worthy of observation; namely, exclusive direction of the attention of a sense—if I may so term it.

We may note this mysterious power in ourselves to a certain extent. In the case of a dog or bird, or any other in which there is little brain work going on to cause distraction, it may be much greater, and account for many wonderful things. It may be said that this is trying to explain the unknown by the even less known; nevertheless, by gathering together many and varied instances of the action of any power some light must be thrown upon it. The mesmerizer seems to deal with this one when he closes all avenues to the senses of his subject except the one he wishes to keep open.

The sense of hearing in some birds seems as wonderful and discriminating as that of smell in dogs. I have watched with astonishment a thrush listening for worms—as their manner is—and very evidently hearing them too, within two yards of a noisy lawn-mower on the other side of a small hedge of roses. Probably the worms came nearer to the surface in consequence of the vibration caused by the machine—they are said to do so—but that the thrush heard and did not see them was evident. Robins appear to be able to distinguish the voices of their own offspring and parents from a number of others, and at a great distance. I say appear, for in such a case one cannot be quite sure, still less can one give all the small details of long-continued observation that make up the evidence in favour of it.

All these cases have a common and mysterious element. It is as if a window were opened in one direction and all others closed; or a chord set vibrating that answers, as a struck glass answers, only to one note; or as if all the available energy were directed along one narrow path. At any rate there is something more than mere keenness of sense.

Sidmouth.

J. M. H.

Electricity of Contact of Gases with Liquids.

WILL you allow me to ask Mr. Enright (NATURE, p. 395) how he proved that the "charge of the escaping hydrogen was positive" or negative, as the case may be? That the escaping spray was electrified by friction, after the manner of the steam spray in Armstrong's old hydro-electric machine, is a natural explanation of these capricious effects; but that gas should be thus electrified, and that this electrification should have any relation whatever to the subject of "atomic charge," are propositions which strike one as improbable.

OLIVER J. LODGE.

The Lunar Eclipse of August 3.

I OBSERVE the account given by "H. H." (p. 367) of the eclipse of the moon as seen at Hamburg on August 3. Here the appearance was certainly unusual; at least I never saw anything like it. The shadow cast on the moon (with a perfectly cloudless sky) was irregular and jagged. I at first thought it was a cloud, but, on looking repeatedly at intervals, I continued to observe the same appearance; allowance being made for the progress of the eclipse. I was prevented by circumstances from

watching continuously, but observed it at a little before 9, and again repeatedly between 9 and 10. M. C.

La Tour de Peilz, August 22.

As seen from Killin, on Loch Tay, the shadow on the moon had no form similar to that given by "H. H.," in your issue of August 18 (p. 367); the sky was clear, and it seems possible that the clouds caused the straight lines shown in the diagram.

H. P. MALET.

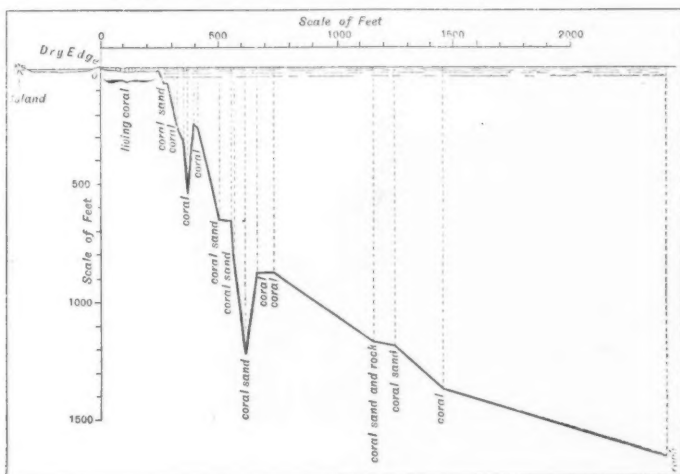
MASÁMARHU ISLAND.

CAPTAIN MACLEAR, commanding H.M.S. *Flying Fish*, obtained, during his voyage home in April last, two sections of the slope of the coral reef surround-

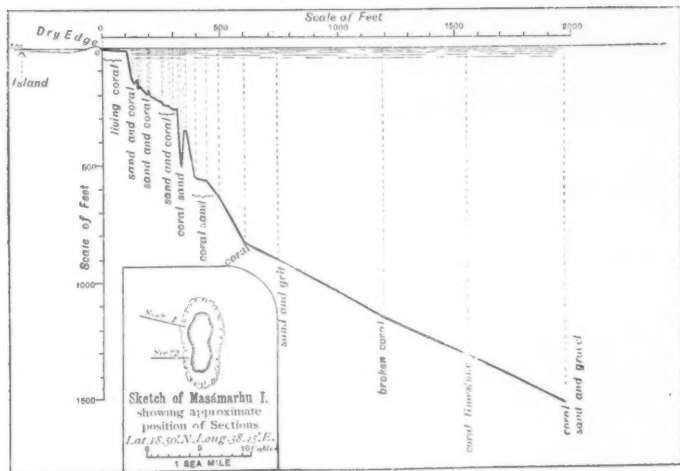
ing the small island of Masámarhu, situated in the Red Sea in lat. $18^{\circ} 49' N.$, long. $38^{\circ} 45' E.$

As accurate sections of reefs standing in deep water are comparatively rare, I have thought that a permanent record of them in the pages of NATURE will render them

SECTIONS OF CORAL REEF OF MASÁMARHU, RED SEA.



No. 1.



No. 2.

more available to those interested. The reduced copies of these sections, appended, show most of the more important features. They are drawn on equal scales vertical and horizontal, showing the true slope. The dotted vertical

lines show where the soundings were obtained. Specimens of coral sand brought home were not from depths sufficient to show the changes of the life on the coral slopes. Mr. John Murray, who has examined them, reports as follows:—

"The fragments of coral belong to *Stylophora palmata*, Blain., a common Red Sea species; and the others to the genera *Stylophora* and *Echinopora*, but too fragmentary for specific determination.

"The beach sand has a mottled red and white appearance. The particles are nearly all rounded, and have an average diameter of 3 or 4 millimetres. They consist of corals, Echinoderms, calcareous Algae, Gasteropod and Lamellibranch shells, and many Foraminifera. Among the latter the following could be recognized: *Paneroplis*

portusus, Forsk.; *Orbitolites complanata*, Lam.; *Rotalia calcar*, d'Orb.; *Amphistegina lessonii*, d'Orb.

"The hardened rock, 'from high-water line near section 2, solid and firm in the sand and similar to the slabs of the south-east shore,' is made up of precisely the same particles as the sand above described, cemented by the infiltration of carbonate of lime among the particles. No mineral particles other than those of organic origin were observed in the sand or hardened slabs."

W. J. L. WHARTON.

THE OWENS COLLEGE NATURAL HISTORY BUILDINGS.

THE recently completed Natural History Museums and Laboratories form an important addition not merely to the Owens College itself but to the teaching appliances of the country at large.

The buildings, which, like the older part of the College, are from the plans of Mr. Alfred Waterhouse, R.A., extend along the north and east sides of the College quadrangle, the main frontage being towards the Oxford Road. They include a lofty central tower and entrance gateway, large and convenient museums for the various departments of

natural history, and a very extensive and well-equipped series of laboratories for zoology, botany, geology, and mineralogy, with lecture-theatres, class-rooms, and private rooms for the professors and demonstrators. The total cost, including fittings, will not be less than £80,000.

The general appearance of the new buildings from the north-east is shown in the illustration.

The Museum block extends along the eastern or Oxford Road frontage, and is approached from the main entrance beneath the central tower; it is also in free communication with the several laboratories. It consists of two main stories, the upper of which has its floor area almost tripled by two very wide galleries, in addition to



Future Extension for
Library and Examination Hall.

Museum Block.

Laboratory Block.

VIEW OF THE NEW BUILDINGS FROM THE OXFORD ROAD.

which there is very extensive storage space in the roof. The ground floor is divided into geological and mineralogical museums, measuring respectively 90 feet by 50 feet, and 65 feet by 26 feet, the former extending along the Oxford Road, the latter facing north, towards Coupland Street. These are lighted from the sides, and will be divided into bays by the main cases, which are placed at right angles to the walls, extending from them to the pillars supporting the roof. In the centre of each bay there will be a large table case, and a smaller one under the window. This arrangement gives at once a maximum of light and a maximum of what is practically wall space; while the division into bays greatly facilitates the classifi-

cation of the collections, and the different forms of case in each bay enable objects of all kinds to be displayed to advantage.

The upper museum, which is approached by a very handsome staircase in the tower, is similarly divided into zoological and botanical portions. It is lighted both from the sides and above, and the general arrangement of the cases will be the same as in the lower museum, with the addition of long rows of table cases round the edges of the galleries. Two large rooms, for use as articulating and preparation rooms, open directly on to the floor of the museum.

Owens College already possesses very important

natural history collections, though owing to want of space it has been impossible up to the present time to arrange or utilize them in a proper manner. The nucleus is formed by the collections previously in the possession of the Manchester Natural History and Geological Societies, which were transferred to the College in 1867 and 1869 respectively: to these, very valuable additions have since been made by gift, bequest, or purchase. The general Geological collection is a very good one; the Tertiary collections, including those made by Prof. Boyd Dawkins and by Mr. Waters, being of exceptional importance, and the Coal Measure series being one of the best in existence. In Mineralogy the David Forbes Collection, which was purchased by the College in 1877, is well known. In Zoology there is a good osteological series; and the collections of shells, including those presented by Mr. Cholmondeley and by Mr. Walton, and of insects are unusually complete, and in exceedingly good condition. The Botanical Museum contains a very fine British herbarium, and Prof. Williamson's unique collections illustrating the Carboniferous flora. The Museum will thus start very fairly equipped, and it may reasonably be hoped that the stimulus caused by the opening of the new buildings will lead to additional gifts and bequests, which will speedily render the collection one worthy in all respects of the College and of the city which has created it.

In the Laboratory block, which occupies the north side of the quadrangle, between the older buildings and the Museum, and is shown in the right-hand corner of the illustration, the ground floor contains on the inner side two lecture-theatres, seating 200 and 80 respectively, with convenient preparation and diagram rooms. On the other side, facing the street, are the mineralogical and petrological laboratories, geological laboratories, geological drawing room, a laboratory for applied geology, and private rooms for the professors and lecturers.

The Botanical Department is on the second floor, and comprises a large laboratory 42 feet by 28 feet, private rooms for the professor and for the demonstrator, and a dark room for physiological experiments. Provision is also made for a greenhouse 20 feet square, in direct connexion with the Laboratory.

The Zoological Laboratories occupy the third and part of the second floor.

The Junior and Senior Laboratories, which are in free communication with each other, measure 42 feet by 37 feet and 42 feet by 16 feet respectively; they are 29 feet high, and are extremely well lighted and equipped. In the Junior Laboratory the tables run north and south; each student has his own locker and drawer at his side, and gas- and water-supply in front of him; larger sinks with hot-water spirals are in the corners of the rooms; a large demonstration-table, with drawers and cupboards beneath, occupies the centre of the room; and a lecture-table and black-board are placed against the north wall. In the Senior Laboratory the tables face north. A gallery runs along the east and west walls of the laboratories, but has not yet been fitted up.

Besides these laboratories there are a Zoological Research Laboratory 42 feet by 16 feet; private rooms for the professor and for the demonstrators; a very convenient tank-room; and large storage space.

The building has concrete floors throughout; the heating is by hot water, and there is a very efficient system of ventilation. At each floor there is free communication between the Laboratory and Museum blocks, and the lift is placed midway between these two.

The Zoological and Botanical Laboratories have been in use since Christmas; the Museum will not be fitted up till October. An excellent opportunity for seeing the buildings is afforded by the meeting of the British Association. The ground-floor museum is being used for the reception-room and post-office, and the upper museum

for reading- and writing-room, ladies' room, smoking-room, &c.; while the quadrangle is occupied by temporary luncheon-rooms and lavatories. The Section Rooms are partly in the College and partly in its immediate neighbourhood.

THE BRITISH ASSOCIATION.

MANCHESTER, Tuesday Evening.

UP to the present the third Manchester meeting of the British Association promises to be as successful as everyone expected it would be. Probably no Local Committee has ever made more strenuous exertions to command success than that which for many months past has been busying itself with preparations for the present meeting. It would be difficult to suggest any improvements on the local preparations. The Reception Room in Owens College is spacious and is entirely confined to business. The Reading Rooms, Ladies' Rooms, Smoking Rooms, and Exhibition Galleries are all upstairs away from the crowd and noise. The Luncheon Rooms can accommodate hundreds, and the Sectional Rooms have had the special care of the Committee, several of whom know well the practical requirements of Sectional work. It is perhaps unfortunate that the rooms for D, E, F, and G are a long way from the Reception Room; but that has been unavoidable. The exhibition in the galleries of the Reading and Writing Room is of special interest. The anthropological collections contributed by Dr. Fritsch, Mr. Coutts Trotter, and others, are extensive and varied and highly instructive. Besides these there are collections of physical instruments by Sir William Thomson and Mr. W. H. Gee, and a fine series of models and apparatus for teaching practical physics in schools and colleges, exhibited by the Owens College Physical Department. In Section C, Prof. Boyd Dawkins exhibits several museum appliances, and Mr. J. H. Teall a series of specimens illustrating his paper on "The Origin of Certain Banded Gneisses." Other exhibits come under Sections D, G, and H, and the whole collection is likely to attract many visitors.

It is not expected that in numbers the present meeting will exceed the Newcastle meeting of 1863, when 3335 persons were present, or even the last meeting in this city in 1861, when the number reached 3138. But of course at present it is impossible to say. Some weeks ago the number who had taken tickets exceeded 2000, and to-day and to-morrow it is probable that at least another 1000 will be added. Whatever may be the number, it is certain that few past meetings of the Association will have surpassed the present in quality and weight. The marked feature is the number of foreign men of science who have promised to attend. The names of most of them have already appeared in NATURE. Their presence is entirely due to the exertions of the Local Committee, and especially, we believe, of Dr. Schuster. Nearly every man of any eminence in science abroad had a cordial letter of invitation to come to Manchester as a guest of the Local Committee, and the result is that over 100 have accepted. Among those who have arrived in Manchester to-day are Prof. Riley, of Washington; Prof. Rowlands, of Baltimore; Prof. Langley, of Michigan; Prof. Dewalque, from Belgium; and Prof. Fittica, of Marburg. Among others who are expected to-morrow I need only mention such names as those of Cleveland Abbe, Neumayer, A. C. Young, Asa Gray, Mendeléeff, Pringsheim, G. Wiedemann, Wislicenus, F. Zirkel, De Bary, Cohn, His, and the two Saportas.

Several important discussions have been arranged for. One between Sections C and D on the arrangement of natural history museums, will be led off by Dr. Woodward on Friday morning. There will be then other discussions in Section D on questions of the greatest scientific interest, while electrolysis will come up again,

I believe, in Sections A and B. A joint discussion on gold and silver has been arranged between Sections C and F. As these discussions will be real, and as several eminent foreigners are expected to take part in them, the meeting on the whole promises to be lively.

The social distractions—*conversazioni*, receptions, dinners, and excursions—are perplexingly numerous. The hand-books for the excursions have been got up with much care and thoughtfulness. There is, indeed, a separate little hand-book for each excursion, the whole set being done up in a case. Another hand-book of about one hundred pages gives an epitome of the history, antiquities, meteorology, physiography, flora and fauna of Manchester and the district.

Thus, so far as the officials are concerned, everything has been done to make the Manchester meeting a success. At the present moment the weather is not quite what could be wished; it is raining hard, and the weather is oppressively sultry. We can only hope it will improve before active operations begin.

INAUGURAL ADDRESS BY SIR HENRY F. ROSCOE, M.P., D.C.L., LL.D., PH.D., F.R.S., V.P.C.S., PRESIDENT.

MANCHESTER, distinguished as the birthplace of two of the greatest discoveries of modern science, heartily welcomes to-day, for the third time, the members and friends of the British Association for the Advancement of Science.

On the occasion of our first meeting in this city in the year 1842, the President, Lord Francis Egerton, commenced his address with a touching allusion to the veteran of science, John Dalton, the great chemist, the discoverer of the laws of chemical combination, the framer of the atomic theory upon which the modern science of chemistry may truly be said to be based. Lord Francis Egerton said:—"Manchester is still the residence of one whose name is uttered with respect wherever science is cultivated, who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge, and to receive from myself, if he will condescend to do so, the expression of my own deep personal regret that increase of years, which to him up to this hour has been but increase of wisdom, should have rendered him, in respect of mere bodily strength, unable to fill on this occasion an office which in his case would have received more honour than it could confer. I do regret that any cause should have prevented the present meeting in his native town from being associated with the name"—and here I must ask you to allow me to exchange the name of Dalton in 1842 for that of Joule in 1887, and to add, again in the words of the President of the former year, that I would gladly have served as a doorkeeper in any house where Joule, the father of science in Manchester, was enjoying his just pre-eminence.

For it is indeed true that the mantle of John Dalton has fallen on the shoulders of one well worthy to wear it, one to whom science owes a debt of gratitude not less than that which it willingly pays to the memory of the originator of the atomic theory. James Prescott Joule it was who, in his determination of the mechanical equivalent of heat, about the very year of our first Manchester meeting, gave to the world of science the results of experiments which placed beyond reach of doubt or cavil the greatest and most far-reaching scientific principle of modern times; namely, that of the conservation of energy. This, to use the words of Tyndall, is indeed a generalization of conspicuous grandeur fit to take rank with the principle of gravitation; more momentous, if that be possible, combining as it does the energies of the material universe into an organic whole, and enabling the eye of science to follow the flying shuttles of the universal power as it weaves what the Erdgeist in "Faust" calls "the living garment of God."

It is well, therefore, for us to remember, in the midst of the turmoil of our active industrial and commercial life, that Manchester not only well represents the energy of England in these practical directions, but that it possesses even higher claims to our regard and respect as being the seat of discoveries of which the value not only to pure science is momentous, but which also lie at the foundation of all our material progress and all our industrial success. For without a knowledge of the laws of chemical combination all the marvellous results with which modern industrial chemistry has astonished the world could not have been achieved, whilst the knowledge of the quantitative

relations existing between the several forms of energy, and the possibility of expressing their amount in terms of ordinary mechanics, are matters which now constitute the life-breath of every branch of applied science. For example, before Dalton's discovery every manufacturer of oil of vitriol—a substance now made each week in thousands of tons within a few miles of this spot—every manufacturer had his own notions of the quantity of sulphur which he ought to burn in order to make a certain weight of sulphuric acid, but he had no idea that only a given weight of sulphur can unite with a certain quantity of oxygen and of water to form the acid, and that an excess of any one of the component parts was not only useless but harmful. Thus, and in tens of thousands of other instances, Dalton replaced rule of thumb by scientific principle. In like manner the applications of Joule's determination of the mechanical equivalent of heat are even more general; the increase and measurement of the efficiency of our steam-engines and the power of our dynamos are only two of the numerous examples which might be adduced of the practical value of Joule's work.

If the place calls up these thoughts, the time of our meeting also awakens memories of no less interest, in the recollection that we this year celebrate the Jubilee of Her Most Gracious Majesty's accession to the throne. It is right that the members of the British Association for the Advancement of Science should do so with heart and voice, for, although science requires and demands no royal patronage, we thereby express the feeling which must be uppermost in the hearts of all men of science, the feeling of thankfulness that we have lived in an age which has witnessed an advance in our knowledge of Nature, and a consequent improvement in the physical, and let us trust also in the moral and intellectual, well-being of the people hitherto unknown; an age with which the name of Victoria will ever be associated.

To give even a sketch of this progress, to trace even in the merest outline the salient points of the general history of science during the fifty momentous years of Her Majesty's reign, is a task far beyond my limited powers. It must suffice for me to point out to you, to the best of my ability, some few of the steps of that progress as evidenced in the one branch of science with which I am most familiar, and with which I am more closely concerned, the science of chemistry.

In the year 1837 chemistry was a very different science from that existing at the present moment. Priestley, it is true, had discovered oxygen, Lavoisier had placed the phenomena of combustion on their true basis, Davy had decomposed the alkalies, Faraday had liquefied many of the gases, Dalton had enunciated the laws of chemical combination by weight, and Gay-Lussac had pointed out the fact that a simple volumetric relation governs the combination of the gases. But we then possessed no knowledge of chemical dynamics, we were then altogether unable to explain the meaning of the heat given off in the act of chemical combination. The atomic theory was indeed accepted, but we were as ignorant of the mode of action of the atoms and as incapable of explaining their mutual relationship as were the ancient Greek philosophers. Fifty years ago, too, the connexion existing between the laws of life, vegetable and animal, and the phenomena of inorganic chemistry, was ill understood. The idea that the functions of living beings are controlled by the same forces, chemical and physical, which regulate the changes occurring in the inanimate world, was then one held by only a very few of the foremost thinkers of the time. Vital force was a term in everyone's mouth, an expression useful, as Goethe says, to disguise our ignorance, for

"Wo d'e Begriffe fehlen."

Da tritt ein Wort zur rechten Zeit sich ein."

Indeed the pioneer of the chemistry of life, Liebig himself, cannot quite shake himself free from the bonds of orthodox opinion, and he who first placed the phenomena of life on a true basis cannot trust his chemical principles to conduct the affairs of the body, but makes an appeal to vital force to help him out of his difficulties; as when in the body politic an unruly mob requires the presence and action of physical force to restrain it and to bring its members under the saving influence of law and order, so too, according to Liebig's views, in the body corporeal a continual conflict between the chemical forces and the vital power occurs throughout life, in which the latter, when it prevails, insures health and a continuance of life, but of which defeat insures disease or death. The picture presented to the student of to-day is a very different one. We now believe that no such conflict is possible, but that life is governed by chemical and physical

forces, even though we cannot in every case explain its phenomena in terms of these forces; that whether these tend to continue or to end existence depends upon their nature and amount, and that disease and death are as much a consequence of the operation of chemical and physical laws as are health and life.

Looking back again to our point of departure fifty years ago, let us for a moment glance at Dalton's labours, and compare his views and those of his contemporaries with the ideas which now prevail. In the first place it is well to remember that the keystone of his atomic theory lies not so much in the idea of the existence and the indivisible nature of the particles of matter—though this idea was so firmly implanted in his mind that, being questioned on one occasion on the subject, he said to his friend the late Mr. Ransome, "Thou knowst it must be so, for no man can split an atom"—as in the assumption that the weights of these particles are different. Thus whilst each of the ultimate particles of oxygen has the same weight as every other particle of oxygen, and each atom of hydrogen, for example, has the same weight as every other particle of hydrogen, the oxygen atom is sixteen times heavier than that of hydrogen, and so on for the atoms of every chemical element, each having its own special weight. It was this discovery of Dalton, together with the further one that the elements combine in the proportions indicated by the relative weights of their atoms or in multiples of these proportions, which at once changed chemistry from a qualitative to a quantitative science, making the old invocation prophetic, "Thou hast ordered all things in measure and number and weight."

The researches of chemists and physicists during the last fifty years have not only strengthened but broadened the foundations of the great Manchester philosopher's discoveries. It is true that his original numbers, obtained by crude and inaccurate methods, have been replaced by more exact figures, but his laws of combination and his atomic explanation of those laws stand as the great bulwarks of our science.

On the present occasion it is interesting to remember that within a stone's-throw of this place is the small room belonging to our Literary and Philosophical Society which served Dalton as his laboratory. Here, with the simplest of all possible apparatus—a few cups, penny ink-bottles, rough balances, and self-made thermometers and barometers—Dalton accomplished his great results. Here he patiently worked, marshalling facts to support his great theory, for as an explanation of his laborious experimental investigations the wise old man says: "Having been in my progress so often misled by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience." Nor ought we when here assembled to forget that the last three of Dalton's experimental essays—one of which, on a new method of measuring water of crystallization, contained more than the germ of a great discovery—were communicated to our Chemical Section in 1842, and that this was the last memorable act of his scientific life. In this last of his contributions to science, as in his first, his method of procedure was that which has been marked out as the most fruitful by almost all the great searchers after Nature's secrets; namely, the assumption of a certain view as a working hypothesis, and the subsequent institution of experiment to bring this hypothesis to a test of reality upon which a legitimate theory is afterwards to be based. "Dalton," as Henry well says, "valued detailed facts mainly, if not solely, as the stepping-stones to comprehensive generalizations."

Next let us ask what light the research of the last fifty years has thrown on the subject of the Daltonian atoms: first, as regards their size; secondly, in respect to their indivisibility and mutual relationships; and thirdly, as regards their motions.

As regards the size and shape of the atoms, Dalton offered no opinion, for he had no experimental grounds on which to form it, believing that they were inconceivably small and altogether beyond the grasp of our senses aided by the most powerful appliances of art. He was in the habit of representing his atoms and their combinations diagrammatically as round disks or spheres made of wood, by means of which he was fond of illustrating his theory. But such mechanical illustrations are not without their danger, for I well remember the answer given by a pupil to a question on the atomic theory: "Atoms are round balls of wood invented by Dr. Dalton." So determinedly indeed did he adhere to his mechanical method of representing the chemical atoms and their combinations that he could not be prevailed upon to adopt the system of chemical formulæ introduced by Berzelius and now universally employed. In a letter addressed

to Graham in April 1837, he writes: "Berzelius's symbols are horrifying. A young student in chemistry might as soon learn Hebrew as make himself acquainted with them." And again: "They appear to me equally to perplex the adepts in science, to discourage the learner, as well as to cloud the beauty and simplicity of the atomic theory."

But modern research has accomplished, as regards the size of the atom, at any rate to a certain extent, what Dalton regarded as impossible. Thus in 1865, Loschmidt, of Vienna, by a train of reasoning which I cannot now stop to explain, came to the conclusion that the diameter of an atom of oxygen or nitrogen was $1/10,000,000$ of a centimetre. With the highest known magnifying power we can distinguish the $1/40,000$ part of a centimetre; if now we imagine a cubic box each of whose sides has the above length, such a box when filled with air will contain from 60 to 100 millions of atoms of oxygen and nitrogen. A few years later William Thomson extended the methods of atomic measurement, and came to the conclusion that the distance between the centres of contiguous molecules is less than $1/5,000,000$ and greater than $1/1,000,000,000$ of a centimetre; or, to put it in language more suited to the ordinary mind, Thomson asks us to imagine a drop of water magnified up to the size of the earth, and then tells us that the coarseness of the graining of such a mass would be something between a heap of small shot and a heap of cricket balls. Or again, to take Clifford's illustration, you know that our best microscopes magnify from 6000 to 8000 times; a microscope which would magnify that result as much again would show the molecular structure of water. Or again, to put it in another form, if we suppose that the minutest organism we can now see were provided with equally powerful microscopes, these beings would be able to see the atoms.

Next, as to the indivisibility of the atom, involving also the question as to the relationships between the atomic weights and properties of the several elementary bodies.

Taking Dalton's aphorism, "Thou knowst no man can split an atom," as expressing the view of the enunciator of the atomic theory, let us see how far this idea is borne out by subsequent work. In the first place, Thomas Thomson, the first exponent of Dalton's generalization, was torn by conflicting beliefs until he found peace in the hypothesis of Prout, that the atomic weights of all the so-called elements are multiples of a common unit, which doctrine he sought to establish, as Thorpe remarks, by some of the very worst quantitative determinations to be found in chemical literature, though here I may add that they were not so incorrect as Dalton's original numbers.

Coming down to a somewhat later date, Graham, whose life was devoted to finding what the motion of an atom was, freed himself from the bondage of the Daltonian aphorism, and defined the atom not as a thing which cannot be divided, but as one which has not been divided. With him, as with Lucretius, as Angus Smith remarks, the original atom may be far down.

But speculative ideas respecting the constitution of matter have been the scientific relaxation of many minds from olden time to the present. In the mind of the early Greek the action of the atom as one substance taking various forms by unlimited combinations was sufficient to account for all the phenomena of the world. And Dalton himself, though upholding the indivisibility of his ultimate particles, says: "We do not know that any of the bodies denominated elementary are absolutely indecomposable." Again, Boyle, treating of the origin of form and quality, says: "There is one universal matter common to all bodies—an extended divisible and impenetrable substance." Then Graham in another place expresses a similar thought when he writes: "It is conceivable that the various kinds of matter now recognized as different elementary substances may possess one and the same ultimate or atomic molecules existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies."

What experimental evidence is now before us bearing upon these interesting speculations? In the first place, then, the space of fifty years has completely changed the face of the inquiry. Not only has the number of distinct well-established elementary bodies increased from fifty-three in 1837 to seventy in 1887 (not including the twenty or more new elements recently said to have been discovered by Krüss and Nilson in certain rare Scandinavian minerals), but the properties of these elements have been studied, and are now known to us with a degree of precision then undreamt of. So that relationships existing between these

bodies which fifty years ago were undiscernible are now clearly manifest, and it is to these relationships that I would for a moment ask your attention. I have already stated that Dalton measured the relative weights of the ultimate particles by assuming hydrogen as the unit, and that Prout believed that on this basis the atomic weights of all the other elements would be found to be multiples of the atomic weight of hydrogen, thus indicating that an intimate constitutional relation exists between hydrogen and all the other elements.

Since the days of Dalton and Prout the truth or otherwise of Prout's law has been keenly contested by the most eminent chemists of all countries. The inquiry is a purely experimental one, and only those who have a special knowledge of the difficulties which surround such inquiries can form an idea of the amount of labour and self-sacrifice borne by such men as Dumas, Stas, and Marignac in carrying out delicate researches on the atomic weights of the elements. What is, then, the result of these most laborious experiments? It is that, whilst the atomic weights of the elements are not exactly either multiples of the unit or of half the unit, many of the numbers expressing most accurately the weight of the atom approximate so closely to a multiple of that of hydrogen, that we are constrained to admit that these approximations cannot be a mere matter of chance, but that some reason must exist for them. What that reason is, and why a close approximation and yet something short of absolute identity exists, is as yet hidden behind the veil; but who is there that doubts that when this Association celebrates its centenary, this veil will have been lifted, and this occult but fundamental question of atomic philosophy shall have been brought into the clear light of day?

But these are by no means all the relationships which modern science has discovered with respect to the atoms of our chemical elements. So long ago as 1829 Döbereiner pointed out that certain groups of elements exist presenting in all their properties strongly marked family characteristics, and this was afterwards extended and insisted upon by Dumas. We find, for example, in the well-known group of chlorine, bromine, and iodine, these resemblances well developed, accompanied moreover by a proportional graduation in their chemical and physical properties. Thus, to take the most important of all their characters, the atomic weight of the middle term is the mean of the atomic weights of the two extremes. But these groups of triads appeared to be unconnected in any way with one another, nor did they seem to bear any relation to the far larger number of the elements not exhibiting these peculiarities.

Things remained in this condition until 1863, when Newlands threw fresh light upon the subject, showing a far-reaching series of relationships. For the first time we thus obtained a glance into the mode in which the elements are connected together, but, like so many new discoveries, this did not meet with the recognition which we now see it deserves. But whilst England thus had the honour of first opening up this new path, it is to Germany and to Russia that we must look for the consummation of the idea. Germany, in the person of Lothar Meyer, keeps, as it is wont to do, strictly within the limits of known facts. Russia, in the person of Mendeleeff, being of a somewhat more imaginative nature, not only seizes the facts which are proved, but ventures upon prophecy. These chemists, amongst whom Carnelley must be named, agree in placing all the elementary bodies in a certain regular sequence, thus bringing to light a periodic recurrence of analogous chemical and physical properties, on account of which the arrangement is termed the periodic system of the elements.

In order to endeavour to render this somewhat complicated matter clear to you, I may perhaps be allowed to employ a simile. Let us, if you please, imagine a series of human families: a French one, represented by Dumas; an English one, by name Newlands; a German one, the family of Lothar Meyer; and lastly, a Russian one, that of Mendeleeff. Let us next imagine the names of these chemists placed in a horizontal line in the order I have mentioned. Then let us write under each the name of his father, and again, in the next lower line, that of his grandfather, followed by that of his great-grandfather, and so on. Let us next write against each of these names the number of years which has elapsed since the birth of the individual. We shall then find that these numbers regularly increase by a definite amount, *i.e.* by the average age of a generation, which will be approximately the same in all the four families. Comparing the ages of the chemists themselves we shall observe certain differences, but these are small in comparison with the period which

has elapsed since the birth of any of their ancestors. Now each individual in this series of family trees represents a chemical element; and just as each family is distinguished by certain idiosyncrasies, so each group of the elementary bodies thus arranged shows distinct signs of consanguinity.

But more than this, it not unfrequently happens that the history and peculiarities of some member of a family may have been lost, even if the memory of a more remote and more famous ancestor may be preserved, although it is clear that such an individual must have had an existence. In such a case Francis Galton would not hesitate from the characteristics of the other members to reproduce the physical and even the mental peculiarities of the missing member; and should genealogical research bring to light the true personal appearance and mental qualities of the man, these would be found to coincide with Galton's estimate.

Such predictions and such verifications have been made in the case of no less than three of our chemical elements. Thus, Mendeleeff pointed out that if, in the future, certain lacunæ in his table were to be filled, they must be filled by elements possessing chemical and physical properties which he accurately specified. Since that time these gaps have actually been stopped by the discovery of gallium by Lecoq de Boisbaudron, of scandium by Nilson, and of germanium by Winkler, and their properties, both physical and chemical, as determined by their discoverers, agree absolutely with those predicted by the Russian chemist. Nay, more than this, we not unfrequently have had to deal with chemical foundlings, elements whose parentage is quite unknown to us. A careful examination of the personality of such waifs has enabled us to restore them to the family from which they have been separated by an unkind fate, and to give them that position in chemical society to which they are entitled.

These remarkable results, though they by no means furnish a proof of the supposition already referred to, viz. that the elements are derived from a common source, clearly point in this direction, and lend some degree of colour to the speculations of those whose scientific imagination, wearying of dry facts, revels in picturing to itself an elemental Bathybius, and in applying to the inanimate, laws of evolution similar to those which rule the animate world. Nor is there wanting other evidence regarding this inquiry, for here heat, the great analyzer, is brought into court. The main portion of the evidence consists in the fact that distinct chemical individuals capable of existence at low temperatures are incapable of existence at high ones, but split up into new materials possessing a less complicated structure than the original. And here it may be well to emphasize the distinction which the chemist draws between the atom and the molecule, the latter being a more or less complicated aggregation of atoms, and especially to point out the fundamental difference between the question of separating the atoms in the molecule and that of splitting up the atom itself. The decompositions above referred to are, in fact, not confined to compound bodies, for Victor Meyer has proved in the case of iodine that the molecule at high temperatures is broken to atoms, and J. J. Thomson has added to our knowledge by showing that this breaking up of the molecule may be effected not only by heat vibrations, but likewise by the electrical discharge at a comparatively low temperature.

How far, now, has this process of simplification been carried? Have the atoms of our present elements been made to yield? To this a negative answer must undoubtedly be given, for even the highest of terrestrial temperatures, that of the electric spark, has failed to shake any one of these atoms in two. That this is the case has been shown by the results with which spectrum analysis, that new and fascinating branch of science, has enriched our knowledge, for that spectrum analysis does give us most valuable aid in determining the varying molecular conditions of matter is admitted by all. Let us see how this bears on the question of the decomposition of the elements, and let us suppose for a moment that certain of our present elements, instead of being distinct substances, were made up of common ingredients, and that these compound elements, if I may be allowed to use so incongruous a term, are split up at the temperature of the electric spark into less complicated molecules. Then the spectroscopic examination of such a body must indicate the existence of these common ingredients by the appearance in the spectra of these elements of identical bright lines. Coincidences of this kind have indeed been observed, but on careful examination these have been shown to be due either to the presence of

some one of the other elements as an impurity, or to insufficient observational power. This absence of coincident lines admits, however, of two explanations—either that the elements are not decomposed at the temperature of the electric spark, or, what appears to me a much more improbable supposition, each one of the numbers of bright lines exhibited by every element indicates the existence of a separate constituent, no two of this enormous number being identical.

Terrestrial analysis having thus failed to furnish favourable evidence, we are compelled to see if any information is forthcoming from the chemistry of the sun and stars. And here I would remark that it is not my purpose now to dilate on the wonders which this branch of modern science has revealed. It is sufficient to remind you that chemists thus have the means placed at their disposal of ascertaining with certainty the presence of elements well known on this earth in fixed stars so far distant that we are now receiving the light which emanated from them perhaps even thousands of years ago.

Since Bunsen and Kirchhoff's original discovery in 1859, the labours of many men of science of all countries have largely increased our knowledge of the chemical constitution of the sun and stars, and to no one does science owe more in this direction than to Lockyer and Huggins in this country, and to Young in the New England beyond the seas. Lockyer has of late years devoted his attention chiefly to the varying nature of the bright lines seen under different conditions of time and place on the solar surface, and from these observations he has drawn the inference that the matching observed by Kirchhoff between, for instance, the iron lines as seen in our laboratories and those visible in the sun, has fallen to the ground. He further explains this want of uniformity by the fact that at the higher transcendental temperatures of the sun the substance which we know here as iron is resolved into separate components. Other experimentalists, however, while accepting Lockyer's facts as to the variations in the solar spectrum, do not admit his conclusions, and would rather explain the phenomena by the well-known differences which occur in the spectra of all the elements when their molecules are subject to change of temperature or change of position.

Further, arguments in favour of this idea of the evolution of the elements have been adduced from the phenomena presented by the spectra of the fixed stars. It is well known that some of these shine with a white, others with a red, and others again with a blue light; and the spectroscopist, especially under the hands of Huggins, has shown that the chemical constitution of these stars is different. The white stars, of which Sirius may be taken as a type, exhibit a much less complicated spectrum than the orange and the red stars; the spectra of the latter remind us more of those of the metalloids and of chemical compounds than of the metals. Hence it has been argued that in the white, presumably the hottest, stars a celestial dissociation of our terrestrial elements may have taken place, whilst in the cooler stars, probably the red, combination even may occur. But even in the white stars we have no direct evidence that a decomposition of any terrestrial atom has taken place; indeed we learn that the hydrogen atom, as we know it here, can endure unscathed the inconceivably fierce temperature of stars presumably many times more fervent than our sun, as Sirius and Vega.

Taking all these matters into consideration, we need not be surprised if the earth-bound chemist should, in the absence of celestial evidence which is incontestable, continue, for the present at least, and until fresh evidence is forthcoming, to regard the elements as the unalterable foundation-stones upon which his science is based.

Pursuing another line of inquiry on this subject, Crookes has added a remarkable contribution to the question of the possibility of decomposing the elements. With his well-known experimental prowess, he has discovered a new and beautiful series of phenomena, and has shown that the phosphorescent lights emitted by certain chemical compounds, especially the rare earths, under an electric discharge in a high vacuum exhibit peculiar and characteristic lines. For the purpose of obtaining his material Crookes started from a substance believed by chemists to be homogeneous, such, for example, as the rare earth yttria, and succeeded by a long series of fractional precipitations in obtaining products which yield different phosphorescent spectra, although when tested by the ordinary methods of what we may term high temperature spectroscopy, they appear to be the one substance employed at the starting-point. The other touchstone by which the identity, or otherwise, of these various products

might be ascertained, viz. the determination of their atomic weights, has not, as yet, engaged Crookes's attention. In explanation of these singular phenomena, the discoverer suggests two possibilities. First, that the bodies yielding the different phosphorescent spectra are different elementary constituents of the substance which we call yttria. Or, if this be objected to because they all yield the same spark-spectrum, he adopts the very reasonable view that the Daltonian atom is probably, as we have seen, a system of chemical complexity; and adds to this the idea that these complex atoms are not all of exactly the same constitution and weight, the differences, however, being so slight that their detection has hitherto eluded our most delicate tests, with the exception of this one of phosphorescence in a vacuum. To these two explanations, Marignac, in a discussion of Crookes's results, adds a third. It having been shown by Crookes himself that the presence of the minutest traces of foreign bodies produce remarkable alterations in the phosphorescent spectra, Marignac suggests that in the course of the thousands of separations which must be made before these differences become manifest, traces of foreign bodies may have been accidentally introduced, or, being present in the original material, may have accumulated to a different extent in the various fractions, their presence being indicated by the only test by which they can now be detected. Which of these three explanations is the true one must be left to future experiment to decide.

We must now pass from the statics to the dynamics of chemistry; that is, from the consideration of the atoms at rest to that of the atoms in motion. Here, again, we are indebted to John Dalton for the first step in this direction, for he showed that the particles of a gas are constantly flying about in all directions; that is, that gases diffuse into one another, as an escape of coal gas from a burner, for example, soon makes itself perceptible throughout the room. Dalton, whose mind was constantly engaged in studying the molecular condition of gases, first showed that a light gas cannot rest upon a heavier gas as oil upon water, but that an interpenetration of each gas by the other takes place. It is, however, to Graham's experiments, made rather more than half a century ago, that we are indebted for the discovery of the law regulating these molecular motions of gases, proving that their relative rates of diffusion are inversely proportional to the square roots of their densities, so that oxygen being 16 times heavier than hydrogen, their relative rates of diffusion are 1 and 4.

But whilst Dalton and Graham indicated that the atoms are in a continual state of motion, it is to Joule that we owe the first accurate determination of the rate of that motion. At the Swansea meeting, in 1848, Joule read a paper before Section A on the "Mechanical Equivalent of Heat and on the Constitution of Elastic Fluids." In this paper Joule remarks that whether we conceive the particles to be revolving round one another according to the hypothesis of Davy, or flying about in every direction according to Herapath's view, the pressure of the gas will be in proportion to the *vis viva* of its particles. "Thus it may be shown that the particles of hydrogen at the barometrical pressure of 30 inches at a temperature of 60° must move with a velocity of 6225·34 feet per second in order to produce a pressure of 14·714 lbs. on the square inch," or, to put it in other words, a molecular cannonade or hailstorm of particles, at the above rate—a rate, we must remember, far exceeding that of a cannon ball—is maintained against the bounding surface.

We can, however, go a step further and calculate with Clerk Maxwell the number of times in which this hydrogen molecule, moving at the rate of 70 miles per minute, strikes against others of the vibrating swarm, and we learn that in one second of time it must knock against others no less than 18 thousand million times.

And here we may pause and dwell for a moment on the reflection that in Nature there is no such thing as great or small, and that the structure of the smallest particle, invisible even to our most searching vision, may be as complicated as that of any one of the heavenly bodies which circle round our sun.

But how does this wonderful atomic motion affect our chemistry? Can chemical science or chemical phenomena throw light upon this motion, or can this motion explain any of the known phenomena of our science? I have already said that Lavoisier left untouched the dynamics of combustion. He could not explain why a fixed and unalterable amount of heat is in most cases emitted, but in some cases absorbed, when chemical combination takes place. What Lavoisier left unexplained Joule has made clear. On August 25, 1843, Joule read a short

communication, I am glad to remember, before the Chemical Section of our Association, meeting that year at Cork, containing an announcement of a discovery which was to revolutionize modern science. This consisted in the determination of the mechanical equivalent of heat, in proving by accurate experiment that by the expenditure of energy equal to that developed by the weight of 772 pounds falling through 1 foot at Manchester, the temperature of 1 pound of water can be raised 1° F. In other words, every change in the arrangement of the particles is accompanied by a definite evolution or an absorption of heat. In all such cases the molecular energy leaves the potential to assume the kinetic form, or *vice versa*. Heat is evolved by the clashing of the atoms, and this amount is fixed and definite.

Thus it is to Joule we owe the foundation of chemical dynamics and the basis of thermal chemistry. As the conservation of mass or the principle of the indestructibility of matter forms the basis of chemical statics, so the principle of the conservation of energy¹ constitutes the foundation of chemical dynamics. Change in the form of matter and change in the form of energy are the universal accompaniments of every chemical operation. Here again it is to Joule we owe the proof of the truth of this principle in another direction, viz. that when electrical energy is developed by chemical change a corresponding quantity of chemical energy disappears. Energy, as defined by Maxwell, is the power of doing work, and work is the act of producing a configuration in a system in opposition to a force which resists that change. Chemical action produces such a change of configuration in the molecules. Hence, as Maxwell says, 'A complete knowledge of the mode in which the potential energy of a system varies with the configuration would enable us to predict every possible motion of the system under the action of given external forces, provided we were able to overcome the purely mathematical difficulties of the calculation.' The object of thermal chemistry is to measure these changes of energy by thermal methods, and to connect these with chemical changes, to estimate the attractions of the atoms and molecules to which the name of chemical affinity has been applied, and thus to solve the most fundamental problem of chemical science. How far has modern research approached the solution of this most difficult problem? How far can we answer the question, What is the amount of the forces at work in these chemical changes? What laws govern these forces? Well, even in spite of the results with which recent researches, especially the remarkable ones of the Danish philosopher Thomsen have enriched us, we must acknowledge that we are yet scarcely in sight of Maxwell's position of successful prediction. Thermal chemistry, we must acknowledge, is even yet in its infancy; it is, however, an infant of sturdy growth, likely to do good work in the world, and to be a credit to him who is its acknowledged father, as well as to those who have so carefully tended it in its early years.

But recent investigation in another direction bids fair even to eclipse the results which have been obtained by the examination of thermal phenomena. And this lies in the direction of electrical chemistry. Faraday's work relating to conductivity of chemical substances has been already referred to, and this has been since substantiated and extended to pure substances by Kohlrausch. It has been shown, for example, that the resistance of absolutely pure water is almost an infinite quantity. But a small quantity of an acid, such as acetic or butyric acid, greatly increases the conductivity; but more than this, it is possible by determination of the conductivity of a mixture of water with these two acids to arrive at a conclusion as to the partition of the molecules of the water between the acids. Such a partition, however, implies a change of position, and therefore we are furnished with a means of recognizing the motion of the molecules in a liquid, and of determining its amount. Thus it has been found that the hindrance to molecular motion is more affected by the chemical character of the liquid than by physical characters such as viscosity. We have seen that chemical change is always accompanied by molecular motion, and further evidence of the truth of this is gained from the extraordinary chemical inactivity of pure unmixed substances. Thus pure anhydrous hydrochloric acid does not act upon lime, whereas the addition of even a trace of moisture sets up a most active chemical change, and hundreds of other examples of a similar kind might be stated. Bearing in mind that these pure anhy-

drous compounds do not conduct, we are led to the conclusion that an intimate relation exists between chemical activity and conductivity. And we need not stop here; for a method is indicated indeed by which it will be possible to arrive at a measure of chemical affinity from determination of conductivity. It has indeed been already shown that the rate of change in the saponification of acetic ether is directly proportional to the conductivity of the liquid employed.

Such wide-reaching inquiries into new and fertile fields, in which we seem to come into nearer touch with the molecular state of matter, and within a measurable distance of accurate mathematical expression, leads to confident hope that Lord Rayleigh's pregnant words at Montreal may ere long be realized: "It is from the further study of electrolysis that we may expect to gain improved views as to the nature of chemical reactions, and of the forces concerned in bringing them about; and I cannot help thinking that the next great advance, of which we already have some foreshadowing, will come on this side."

There is, perhaps, no branch of our science in which the doctrine of the Daltonian atom plays a more conspicuous part than in organic chemistry or the chemistry of the carbon compounds, as there is certainly none in which such wonderful progress has been made during the last fifty years. One of the most striking and perplexing discoveries made rather more than half a century ago was that chemical compounds could exist which, whilst possessing an identical chemical composition, that is containing the same percentage quantity of their constituents, are essentially distinct chemical substances exhibiting different properties. Dalton was the first to point out the existence of such substances, and to suggest that the difference was to be ascribed to a different or to a multiple arrangement of the constituent atoms. Faraday soon afterwards proved that this supposition was correct, and the research of Liebig and Wöhler on the identity of composition of the salts of fulminic and cyanic acid gave further confirmation to the conclusion, leading Faraday to remark that "now we are taught to look for bodies composed of the same elements in the same proportion, but differing in their qualities, they may probably multiply upon us." How true this prophecy has become we may gather from the fact that we now know of thousands of cases of this kind, and that we are able not only to explain the reason of their difference by virtue of the varying position of the atoms within the molecule, but even to predict the number of distinct variations in which any given chemical compound can possibly exist. How large this number may become may be understood from the fact that, for example, one chemical compound, a hydrocarbon containing thirteen atoms of carbon combined with twenty-eight atoms of hydrogen, can be shown to be capable of existing in no less than 802 distinct forms.

Experiment in every case in which it has been applied has proved the truth of such a prediction, so that the chemist has no need to apply the cogent argument sometimes said to be used by experimentalists enamoured of pet theories, "When facts do not agree with theory, so much the worse for the facts"! This power of successful prediction constitutes a high-water mark in science, for it indicates that the theory upon which such a power is based is a true one.

But if the Daltonian atom forms the foundation of this theory, it is upon a knowledge of the mode of arrangement of these atoms and on a recognition of their distinctive properties that the superstructure of modern organic chemistry rests. Certainly it does appear almost to verge on the miraculous that chemists should now be able to ascertain with certainty the relative position of atoms in a molecule so minute that millions upon millions, like the angels in the schoolmen's discussion, can stand on a needle's point. And yet this process of orientation is one which is accomplished every day in our laboratories, and one which more than any other has led to results of a startling character. Still, this sword to open the oyster of science would have been wanting to us if we had not taken a step farther than Dalton did, in the recognition of the distinctive nature of the elemental atoms. We now assume on good grounds that the atom of each element possesses distinct capabilities of combination: some a single capability, others a double, others a triple, and others again a fourfold combining capacity. The germs of this theory of valency, one of the most fruitful of modern chemical ideas, were enunciated by Frankland in 1852, but the definite explanation of the linking of atoms, of the tetrad nature of the carbon atoms, their power of combination, and of the difference in structure between the fatty and aromatic series of compounds,

¹ "The total energy of any material system is a quantity which can neither be increased nor diminished by any action between the parts of the system, though it may be transformed into any of the forms of which energy is susceptible."—MAXWELL.

was first pointed out by Kekulé in 1857; though we must not forget that this great principle was fore-shadowed so long ago as 1833 from a physical point of view by Faraday in his well-known laws of electrolysis, and that it is to Helmholtz, in his celebrated Faraday Lecture, that we owe the complete elucidation of the subject; for, whilst Faraday has shown that the number of the atoms electrolytically deposited is in the inverse ratio of their valencies, Helmholtz has explained this by the fact that the quantity of electricity with which each atom is associated is directly proportional to its valency.

Amongst the tetrad class of elements, carbon, the distinctive element of organic compounds, finds its place; and the remarkable fact that the number of carbon compounds far exceeds that of all the other elements put together receives its explanation. For these carbon atoms not only possess four means of grasping other atoms, but these four-handed carbon atoms have a strong partiality for each other's company, and readily attach themselves hand in hand to form open chains or closed rings, to which the atoms of other elements join to grasp the unoccupied carbon hand, and thus to yield a dancing company in which all hands are locked together. Such a group, each individual occupying a given position with reference to the others, constitutes the organic molecule. When, in such a company, the individual members change hands, a new combination is formed. And as in such an assembly the eye can follow the changing positions of the individual members, so the chemist can recognize in his molecule the position of the several atoms, and explain by this the fact that each arrangement constitutes a new chemical compound possessing different properties, and account in this way for the decompositions which each differently constituted molecule is found to undergo.

Chemists are, however, not content with representing the arrangement of the atoms in one plane, as on a sheet of paper, but attempt to express the position of the atoms in space. In this way it is possible to explain certain observed differences in isomeric bodies, which otherwise baffled our efforts. To Van t'Hoff, in the first instance, and more recently to Wislicenus, chemistry is indebted for work in this direction, which throws light on hitherto obscure phenomena, and points the way to still further and more important advances.

It is this knowledge of the mode in which the atoms in the molecule are arranged, this power of determining the nature of this arrangement, which has given to organic chemistry the impetus which has overcome so many experimental obstacles, and given rise to such unlooked-for results. Organic chemistry has now become synthetic. In 1837 we were able to build up but very few and very simple organic compounds from their elements; indeed the views of chemists were much divided as to the possibility of such a thing. Both Gmelin and Berzelius argued that organic compounds, unlike inorganic bodies, cannot be built up from their elements. Organic compounds were generally believed to be special products of the so-called vital force, and it was only intuitive minds like those of Liebig and Wöhler who foresaw what was coming, and wrote in 1837 strongly against this view, asserting that the artificial production in our laboratories of all organic substances, so far as they do not constitute a living organism, is not only probable but certain. Indeed, they went a step farther, and predicted that sugar, morphia, salicine, will all thus be prepared; a prophecy which, I need scarcely remind you, has been after fifty years fulfilled, for at the present time we can prepare an artificial sweetening principle, an artificial alkaloid, and salicine.

In spite of these predictions, and in spite of Wöhler's memorable discovery in 1828 of the artificial production of urea, which did in reality break down for ever the barrier of essential chemical difference between the products of the inanimate and the animate world, still, even up to a much later date, contrary opinions were held, and the synthesis of urea was looked upon as the exception which proves the rule. So it came to pass that for many years the artificial production of any of the more complicated organic substances was believed to be impossible. Now the belief in a special vital force has disappeared like the *ignis fatuus*, and no longer lures us in the wrong direction. We know now that the same laws regulate the formation of chemical compounds in both animate and inanimate nature, and the chemist only asks for a knowledge of the constitution of any definite chemical compound found in the organic world in order to be able to promise to prepare it artificially.

But the progress of synthetic organic chemistry, which has of late been so rapid, was made in the early days of the half-century

only by feeble steps and slow. Seventeen long years elapsed between Wöhler's discovery and the next real synthesis. This was accomplished by Kolbe, who in 1845 prepared acetic acid from its elements. But then a splendid harvest of results gathered in by chemists of all nations quickly followed, a harvest so rich and so varied that we are apt to be overpowered by its wealth, and amidst so much that is alluring and striking we may well find it difficult to choose the most appropriate examples for illustrating the power and the extent of modern chemical synthesis.

Next, as a contrast to our picture, let us for a moment glance back again to the state of things fifty years ago, and then notice the chief steps by which we have arrived at our present position. In 1837 organic chemistry possessed no scientific basis, and therefore no classification of a character worthy of the name. Writing to Berzelius in that year, Wöhler describes the condition of organic chemistry as one enough to drive a man mad. "It seems to me," says he, "like the tropical forest primæval, full of the strangest growths, an endless and pathless thicket in which a man may well dread to wander." Still clearances had already been made in this wilderness of facts. Berzelius in 1832 welcomed the results of Liebig and Wöhler's re-earch on benzoic acid as the dawn of a new era; and such it really was, inasmuch as it introduced a novel and fruitful idea—namely, the possibility of a group of atoms acting like an element by pointing out the existence of organic radicals. This theory was strengthened and confirmed by Bunsen's classical researches on the cacodyl compounds, in which he showed that a common group of elements which acts exactly as a metal can exist in the free state, and this was followed soon afterwards by isolation of the so-called alcohol radicals by Frankland and Kolbe. It is, however, to Schorlemmer that we owe our knowledge of the true constitution of these bodies, a matter which proved to be of vital importance for the further development of the science.

Turning our glance in another direction we find that Dumas in 1834 by this law of substitution threw light upon a whole series of singular and unexplained phenomena by showing that an exchange can take place between the constituent atoms in a molecule. Laurent indeed went farther, and assumed that a chlorine atom, for example, took up the position vacated by an atom of hydrogen and played the part of its displaced rival, so that the chemical and physical properties of the substitution-product were thought to remain substantially the same as those of the original body. A singular story is connected with this discovery. At a *soirée* in the Tuileries in the time of Charles X. the guests were almost suffocated by acrid vapours which were evidently emitted by the burning wax candles, and the great chemist Dumas was called in to examine into the cause of the annoyance. He found that the wax of which the candles were made had been bleached by chlorine, that a replacement of some of the hydrogen atoms of the wax by chlorine had occurred, and that the suffocating vapours consisted of hydrochloric acid given off during the combustion. The wax was as white and as odourless as before, and the fact of the substitution of chlorine for hydrogen could only be recognized when the candles were destroyed by burning. This incident induced Dumas to investigate more closely this class of phenomena, and the results of this investigation are embodied in his law of substitution. So far indeed did the interest of the French school of chemists lead them that some assumed that not only the hydrogen but also the carbon of organic bodies could be replaced by substitution. Against this idea Liebig protested, and in a satirical vein he informs the chemical public, writing from Paris under the *nom de plume* of S. Windler, that he has succeeded in substituting not only the hydrogen but the oxygen and carbon in cotton cloth by chlorine, and he adds that the London shops are now selling nightcaps and other articles of apparel made entirely of chlorine, goods which meet with much favour, especially for hospital use!

But the debt which chemistry, both inorganic and organic, thus owes to Dumas' law of substitution is serious enough, for it proved to be the germ of Williamson's classical researches on etherification, as well as of those of Wurtz and Hofmann on the compound ammonias, investigations which lie at the base of the structure of modern chemistry. Its influence has been, however, still more far-reaching, inasmuch as upon it depends in great measure the astounding progress made in the wide field of organic synthesis.

It may here be permitted to me to sketch in rough outline the

principles upon which all organic syntheses have been effected. We have already seen that as soon as the chemical structure of a body has been ascertained its artificial preparation may be certainly anticipated, so that the first step to be taken is the study of the structure of the naturally occurring substance which it is desired to prepare artificially by resolving it into simpler constituents, the constitution of which is already known. In this way, for example, Hofmann discovered that the alkaloid coniine, the poisonous principle of hemlock, may be decomposed into a simpler substance well known to chemists under the name of pyridine. This fact having been established by Hofmann, and the grouping of the atoms approximately determined, it was then necessary to reverse the process, and, starting with pyridine, to build up a compound of the required constitution and properties, a result recently achieved by Ladenburg in a series of brilliant researches. The well-known synthesis of the colouring matter of madder by Graebe and Liebermann, preceded by the important researches of Schunck, and that of indigo by Baeyer, are other striking examples in which this method has been successfully followed.

Not only has this intimate acquaintance with the changes which occur within the molecules of organic compounds been utilized, as we have seen, in the synthesis of naturally occurring substances, but it has also led to the discovery of many new ones. Of these perhaps the most remarkable instance is the production of an artificial sweetening agent termed saccharin, 250 times sweeter than sugar, prepared by a complicated series of reactions from coal-tar. Nor must we imagine that these discoveries are of scientific interest only, for they have given rise to the industry of the coal-tar colours, the value of which is measured by millions sterling annually, an industry which Englishmen may be proud to remember was founded by our countryman Perkin.

Another interesting application of synthetic chemistry to the needs of every-day life is the discovery of a series of valuable febrifuges, amongst which I may mention antipyrin as the most useful. An important aspect in connexion with the study of these bodies is the physiological value which has been found to attach to the introduction of certain organic radicals, so that an indication is given of the possibility of preparing a compound which will possess certain desired physiological properties, or even to foretell the kind of action which such bodies may exert on the animal economy.

But it is not only the physiological properties of chemical compounds which stand in intimate relation with their constitution, for we find that this is the case with all their physical properties. It is true that at the beginning of our period any such relation was almost unsuspected, whilst at the present time the number of instances in which this connexion has been ascertained is almost infinite. Amongst these perhaps the most striking is the relationship which has been pointed out between the optical properties and chemical composition. This was in the first place recognized by Pasteur in his classical researches on racemic and tartaric acids in 1848; but the first to indicate a quantitative relationship and a connexion between chemical structure and optical properties was Gladstone in 1863. Great instrumental precision has been brought to bear on this question, and consequently most important practical applications have resulted. I need only refer to the well-known accurate methods now in every-day use for the determination of sugar by the polariscope, equally valuable to the physician and to the manufacturer.

But now the question may well be put, is any limit set to this synthetic power of the chemist? Although the danger of dogmatizing as to the progress of science has already been shown in too many instances, yet one cannot help feeling that the barrier which exists between the organized and unorganized worlds is one which the chemist at present sees no chance of breaking down.

It is true that there are those who profess to foresee that the day will arrive when the chemist, by a succession of constructive efforts, may pass beyond albumen, and gather the elements of lifeless matter into a living structure. Whatever may be said regarding this from other standpoints, the chemist can only say that at present no such problem lies within his province. Protoplasm, with which the simplest manifestations of life are associated, is not a compound, but a structure built up of compounds. The chemist may successfully synthesize any of its component molecules, but he has no more reason to look forward to the synthetic production of the structure than to imagine that

the synthesis of gallic acid leads to the artificial production of gall-nuts.

Although there is thus no prospect of our effecting a synthesis of organized material, yet the progress made in our knowledge of the chemistry of life during the last fifty years has been very great, and so much so indeed that the sciences of physiological and of pathological chemistry may be said to have entirely arisen within this period.

In the introductory portion of this address I have already referred to the relations supposed to exist fifty years ago between vital phenomena and those of the inorganic world. Let me now briefly trace a few of the more important steps which have marked the progress of this branch of science during this period. Certainly no portion of our science is of greater interest, nor, I may add, of greater complexity, than that which, bearing on the vital functions both of plants and of animals, endeavours to unravel the tangled skein of the chemistry of life, and to explain the principles according to which our bodies live, and move, and have their being. If, therefore, in the less complicated problems with which other portions of our science have to deal, we find ourselves, as we have seen, often far from possessing satisfactory solutions, we cannot be surprised to learn that with regard to the chemistry of the living body—whether vegetable or animal—in health or disease we are still farther from a complete knowledge of phenomena, even those of fundamental importance.

It is of interest here to recall the fact that nearly fifty years ago Liebig presented to the Chemical Section of this Association a communication in which, for the first time, an attempt was made to explain the phenomena of life on chemical and physical lines, for in this paper he admits the applicability of the great principle of the conservation of energy to the functions of animals, pointing out that the animal cannot generate more heat than is produced by the combustion of the carbon and hydrogen of his food.

"The source of animal heat," says Liebig, "has previously been ascribed to nervous action or to the contraction of the muscles, or even to the mechanical motions of the body, as if these motions could exist without an expenditure of force [equal to that] consumed in producing them." Again he compares the living body to a laboratory furnace in which a complicated series of changes occur in the fuel, but in which the end-products are carbonic acid and water, the amount of heat evolved being dependent, not upon the intermediate, but upon the final products. Liebig asked himself the question, Does every kind of food go to the production of heat; or can we distinguish, on the one hand, between the kind of food which goes to create warmth, and, on the other, that by the oxidation of which the motions and mechanical energy of the body are kept up? He thought that he was able to do this, and he divided food into two categories. The starchy or carbohydrate food is that, said he, which by its combustion provides the warmth necessary for the existence and life of the body. The albuminous or nitrogenous constituents of our food, the flesh meat, the gluten, the casein out of which our muscles are built up, are not available for the purposes of creating warmth, but it is by the waste of those muscles that the mechanical energy, the activity, the motions of the animal are supplied. We see, said Liebig, that the Esquimaux feeds on fat and tallow, and this burning in his body keeps out the cold. The Gaucho, riding on the pampas, lives entirely on dried meat, and the rowing man and pugilist, trained on beefsteaks and porter, require little food to keep up the temperature of their bodies, but much to enable them to meet the demand for fresh muscular tissue, and for this purpose they need to live on a strongly nitrogenous diet.

Thus far Liebig. Now let us turn to the present state of our knowledge. The question of the source of muscular power is one of the greatest interest, for, as Frankland observes, it is the corner-stone of the physiological edifice and the key to the nutrition of animals.

Let us examine by the light of modern science the truth of Liebig's view—even now not uncommonly held—as to the functions of the two kinds of food, and as to the cause of muscular exercise being the oxidation of the muscular tissue. Soon after the promulgation of these views, J. R. Mayer, whose name as the first expositor of the idea of the conservation of energy is so well known, warmly attacked them, throwing out the hypothesis that all muscular action is due to the combustion of food, and not to the destruction of muscle, proving his case by showing that if the muscles of the heart be destroyed in doing

mechanical work the heart would be burnt up in eight days! What does modern research say to this question? Can it be brought to the crucial test of experiment? It can; but how? Well, in the first place we can ascertain the work done by a man or any other animal; we can measure this work in terms of our mechanical standard, in kilogramme-metres or foot-pounds. We can next determine what is the destruction of nitrogenous tissue at rest and under exercise by the amount of nitrogenous material thrown off by the body. And here we must remember that these tissues are never completely burnt, so that free nitrogen is never eliminated. If now we know the heat-value of the burnt muscle, it is easy to convert this into its mechanical equivalent, and thus measure the energy generated. What is the result? Is the weight of muscle destroyed by ascending the Faulhorn or by working on the treadmill sufficient to produce on combustion heat enough when transformed into mechanical exercise to lift the body up to the summit of the Faulhorn or to do the work on the treadmill? Careful experiment has shown that this is so far from being the case that the actual energy developed is twice as great as that which could possibly be produced by the oxidation of the nitrogenous constituents eliminated from the body during twenty-four hours. That is to say, taking the amount of nitrogenous substance cast off from the body, not only whilst the work was being done but during twenty-four hours, the mechanical effect capable of being produced by the muscular tissue from which this cast-off material is derived would only raise the body half-way up the Faulhorn, or enable the prisoner to work half his time on the treadmill.

Hence it is clear that Liebig's proposition is not true. The nitrogenous constituents of the food do doubtless go to repair the waste of muscle, which, like every other portion of the body, needs renewal, whilst the function of the non-nitrogenous food is not only to supply the animal heat, but also to furnish, by its oxidation, the muscular energy of the body.

We thus come to the conclusion that it is the potential energy of the food which furnishes the actual energy of the body, expressed in terms either of heat or of mechanical work.

But there is one other factor which comes into play in this question of mechanical energy, and must be taken into account; and this factor we are as yet unable to estimate in our usual terms. It concerns the action of the mind upon the body, and, although incapable of exact expression, exerts none the less an important influence on the physics and chemistry of the body, so that a connexion undoubtedly exists between intellectual activity or mental work and bodily nutrition. In proof that there is a marked difference between voluntary and involuntary work, we need only compare the mechanical action of the heart, which never causes fatigue, with that of the voluntary muscles, which become fatigued by continued exertion. So, too, we know well that an amount of drill which is fatiguing to the recruit is not felt by the old soldier, who goes through the evolutions automatically. What is the expenditure of mechanical energy which accompanies mental effort, is a question which science is probably far removed from answering. But that the body experiences exhaustion as the result of mental activity is a well-recognized fact. Indeed, whilst the second law of thermodynamics teaches that in none of the mechanical contrivances for the conversion of heat into actual energy can such a conversion be complete, it is perhaps possible, as Helmholtz has suggested, that such a complete conversion may take place in the subtle mechanism of the animal organism.

The phenomena of vegetation, no less than those of the animal world, have, however, during the last fifty years been placed by the chemist on an entirely new basis. Although before the publication of Liebig's celebrated report on chemistry and its application to agriculture, presented to the British Association in 1840, much had been done, many fundamental facts had been established, still Liebig's report marks an era in the progress of this branch of our science. He not only gathered up in a masterly fashion the results of previous workers, but put forward his own original views with a boldness and frequently with a sagacity which gave a vast stimulus and interest to the questions at issue. As a proof of this I may remind you of the attack which he made on, and the complete victory which he gained over, the humus theory. Although Saussure and others had already done much to destroy the basis of this theory, yet the fact remained that vegetable physiologists up to 1840 continued to hold to the opinion that humus, or decayed vegetable matter, was the only source of the carbon of vegetation. Liebig, giving due consideration to the labours of Saussure, came to the con-

clusion that it was absolutely impossible that the carbon deposited as vegetable tissue over a given area, as for instance over an area of forest land, could be derived from humus, which is itself the result of the decay of vegetable matter. He asserted that the whole of the carbon of vegetation is obtained from the atmospheric carbonic acid, which, though only present in the small relative proportion of 4 parts in 10,000 of air, is contained in such absolutely large quantity that if all the vegetation on the earth's surface were burnt, the proportion of carbonic acid which would thus be thrown into the air would not be sufficient to double the present amount.

That this conclusion of Liebig's is correct needed experimental proof, but such proof could only be given by long-continued and laborious experiment, and this serves to show that chemical research is not now confined to laboratory experiments lasting perhaps a few minutes, but that it has invaded the domain of agriculture as well as of physiology, and reckons the periods of her observations in the field not by minutes, but by years. It is to our English agricultural chemists Lawes and Gilbert that we owe the complete experimental proof required. And it is true that this experiment was a long and tedious one, for it has taken forty-four years to give the definite reply. At Rothamsted a plot was set apart for the growth of wheat. For forty-four successive years that field has grown wheat without addition of any carbonized manure; so that the only possible source from which the plant could obtain the carbon for its growth is the atmospheric carbonic acid. Now, the quantity of carbon which on an average was removed in the form of wheat and straw from a plot manured only with mineral matter was 1000 pounds, whilst on another plot, for which a nitrogenous manure was employed, 1500 pounds more carbon was annually removed; or 2500 pounds of carbon are removed by this crop annually without the addition of any carbonaceous manure. So that Liebig's prevision has received a complete experimental verification.

May I without wearying you with experimental details refer for a moment to Liebig's views as to the assimilation of nitrogen by plants—a much more complicated and difficult question than the one we have just considered—and compare these with the most modern results of agricultural chemistry? We find that in this case his views have not been substantiated. He imagined that the whole of the nitrogen required by the plant was derived from atmospheric ammonia; whereas Lawes and Gilbert have shown by experiments of a similar nature to those just described, and extending over a nearly equal length of time, that this source is wholly insufficient to account for the nitrogen removed in the crop, and have come to the conclusion that the nitrogen must have been obtained either from a store of nitrogenous material in the soil or by absorption of free nitrogen from the air. These two apparently contradictory alternatives may perhaps be reconciled by the recent observations of Warington and of Berthelot, which have thrown light upon the changes which the so-called nitrogenous capital of the soil undergoes, as well as upon its chemical nature, for the latter has shown that under certain conditions the soil has the power of absorbing the nitrogen of the air, forming compounds which can subsequently be assimilated by the plant.

Touching us as human beings even still more closely than the foregoing, is the influence which chemistry has exerted on the science of pathology, and in no direction has greater progress been made than in the study of micro-organisms in relation to health and disease. In the complicated chemical changes to which we give the names of fermentation and putrefaction, the views of Liebig, according to which these phenomena are of a purely chemical character, have given way under the searching investigations of Pasteur, who established the fundamental principle that these processes are inseparably connected with the life of certain low forms of organisms. Thus was founded the science of bacteriology, which in Lister's hands has yielded such splendid results in the treatment of surgical cases; and in those of Klebs, Koch, William Roberts, and others, has been the means of detecting the cause of many diseases both in man and animals; the latest and not the least important of which is the remarkable series of successful researches by Pasteur into the nature and mode of cure of that most dreadful of maladies, hydrophobia. And here I may be allowed to refer with satisfaction to the results of the labours on this subject of a Committee the formation of which I had the honour of moving for in the House of Commons. These results confirm in every respect Pasteur's assertions, and prove beyond a doubt that the adoption of his method has prevented the occurrence of hydrophobia in a large proportion of persons

bitten by rabid animals, who, if they had not been subjected to this treatment would have died of that disease. The value of his discovery is, however, greater than can be estimated by its present utility, for it shows that it may be possible to avert other diseases besides hydrophobia by the adoption of a somewhat similar method of investigation and of treatment. This, though the last, is certainly not the least of the debts which humanity owes to the great French experimentalist. Here it might seem as if we had outstepped the boundaries of chemistry, and have to do with phenomena purely vital. But recent research indicates that this is not the case, and points to the conclusion that the microscopist must again give way to the chemist, and that it is by chemical rather than by biological investigation that the causes of diseases will be discovered, and the power of removing them obtained. For we learn that the symptoms of infective diseases are no more due to the microbes which constitute the infection than alcoholic intoxication is produced by the yeast-cell, but that these symptoms are due to the presence of definite chemical compounds, the result of the life of these microscopic organisms. So it is to the action of these poisonous substances formed during the life of the organism, rather than to that of the organism itself, that the special characteristics of the disease are to be traced; for it has been shown that the disease can be communicated by such poisons in entire absence of living organisms.

If I have thus far dwelt on the progress made in certain branches of pure science it is not because I undervalue the other methods by which the advancement of science is accomplished, viz. that of the application and of the diffusion of a knowledge of Nature, but rather because the British Association has always held, and wisely held, that original investigation lies at the root of all application, so that to foster its growth and encourage its development has for more than fifty years been our chief aim and wish.

Had time permitted I should have wished to have illustrated this dependence of industrial success upon original investigation, and to have pointed out the prodigious strides which chemical industry in this country has made during the fifty years of Her Majesty's reign. As it is I must be content to remind you how much our modern life, both in its artistic and useful aspects, owes to chemistry, and, therefore, how essential a knowledge of the principles of the science is to all who have the industrial progress of the country at heart.

This leads me to refer to what has been accomplished in this country of ours towards the diffusion of scientific knowledge amongst the people during the Victorian era. It is true that the English people do not possess, as yet, that appreciation of the value of science so characteristic of some other nations. Up to very recent years our educational system, handed down to us from the Middle Ages, has systematically ignored science, and we are only just beginning, thanks in a great degree to the provision of the late Prince Consort, to give it a place, and that but an unimportant one, in our primary and secondary schools or in our Universities. The country is, however, now awakening to the necessity of placing its house in order in this respect, and is beginning to see that if she is to maintain her commercial and industrial supremacy the education of her people from top to bottom must be carried out on new lines. The question as to how this can be most safely and surely accomplished is one of transcendent national importance, and the statesman who solves this educational problem will earn the gratitude of generations yet to come.

In conclusion, may I be allowed to welcome the unprecedentedly large number of foreign men of science who have on this occasion honoured the British Association by their presence, and to express the hope that this meeting may be the commencement of an international scientific organization, the only means nowadays existing, to use the words of one of the most distinguished of our guests, of establishing that fraternity among nations from which politics appear to remove us further and further by absorbing human powers and human work, and directing them to purposes of destruction. It would indeed be well if Great Britain, which has hitherto taken the lead in so many things that are great and good, should now direct her attention to the furthering of international organizations of a scientific nature. A more appropriate occasion than the present meeting could perhaps hardly be found for the inauguration of such a movement.

But whether this hope be realized or not, we all unite in that one great object, the search after truth for its own sake, and

we all, therefore, may join in re-echoing the words of Lessing:—"The worth of man lies not in the truth which he possesses, or believes that he possesses, but in the honest endeavour which he puts forth to secure that truth; for not by the possession of truth, but by the search after it are the faculties of man enlarged, and in this alone consists his ever-growing perfection. Possession fosters content, indolence, and pride. If God should hold in His right hand all truth, and in His left hand the ever-active desire to seek truth, though with the condition of perpetual error, I would humbly ask for the contents of the left hand, saying, 'Father, give me this; pure truth is only for Thee.'"

SECTION A.

MATHEMATICAL AND PHYSICAL SCIENCE.

OPENING ADDRESS BY SIR ROBERT S. BALL, LL.D., F.R.S.,
PRESIDENT OF THE SECTION.

A Dynamical Parable.

THE subject I have chosen for my address to you to-day has been to me a favourite topic of meditation for many years. It is that part of the science of theoretical mechanics which is usually known as the "Theory of Screws."

A good deal has been already written on this theory, but I may say with some confidence that the aspect in which I shall invite you now to look at it is a novel one. I propose to give an account of the proceedings of a committee appointed to investigate and experiment upon certain dynamical phenomena. It may appear to you that the experiments I shall describe have not as yet been made, that even the committee itself has not as yet been called together. I have accordingly ventured to call this address "A Dynamical Parable."

There was once a rigid body which lay peacefully at rest. A committee of natural philosophers was appointed to make an experimental and rational inquiry into the dynamics of that body. The committee received special instructions. They were to find out why the body remained at rest, notwithstanding that certain forces were in action. They were to apply impulsive forces and observe how the body would begin to move. They were also to investigate the small oscillations. These being settled, they were then to—But here the chairman interposed; he considered that for the present, at least, there was sufficient work in prospect. He pointed out how the questions already proposed just completed a natural group. "Let it suffice for us," he said, "to experiment upon the dynamics of this body so long as it remains in or near to the position it now occupies. We may leave to some more ambitious committee the task of following the body in all conceivable gyrations through the universe."

The committee was judiciously chosen. Mr. Anharmonic undertook the geometry. He was found to be of the utmost value in the more delicate parts of the work, though his colleagues thought him rather prosy at times. He was much aided by his two friends, Mr. One-to-One, who had charge of the homographic department, and Mr. Helix, whose labours will be seen to be of much importance. As a most respectable if rather old-fashioned member, Mr. Cartesian was added to the committee, but his antiquated tactics were quite out-manoeuvred by those of Helix and One-to-One. I need only mention two more names. Mr. Commonsense was, of course, present as an *ex-officio* member, and valuable service was even rendered by Mr. Querulous, who objected at first to serve on the committee at all. He said that the inquiry was all nonsense, because everybody knew as much as they wished to know about the dynamics of a rigid body. The subject was as old as the hills, and had all been settled long ago. He was persuaded, however, to look in occasionally. It will appear that a remarkable result of the labours of the committee was the conversion of Mr. Querulous himself.

The committee assembled in the presence of the rigid body to commence their memorable labours. There was the body at rest, a huge amorphous mass, with no regularity in its shape—no uniformity in its texture. But what chiefly alarmed the committee was the bewildering nature of the constraints by which the movements of the body were hampered. They had been accustomed to nice mechanical problems, in which a smooth body lay on a smooth table, or a wheel rotated on an axle, or a body rotated around a point. In all these cases the constraints

were of a simple character, and the possible movements of the body were obvious. But the constraints in the present case were of puzzling complexity. There were cords and links, moving axes, surfaces with which the body lay in contact, and many other geometrical constraints. Experience of ordinary problems in mechanics would be of little avail. In fact, the chairman truly appreciated the situation when he said that the constraints were of a perfectly general type.

In the dismay with which this announcement was received Mr. Commonsense advanced to the body and tried whether it could move at all. Yes, it was obvious that in some ways the body could be moved. Then said Commonsense, "Ought we not first to study carefully the nature of the freedom which the body possesses? Ought we not to make an inventory of every distinct movement of which the body is capable? Until this has been obtained I do not see how we can make any progress in the dynamical part of our business."

Mr. Querulous ridiculed this proposal. "How could you," he said, "make any geometrical theory of the mobility of a body without knowing all about the constraints? And yet you are attempting to do so with perfectly general constraints of which you know nothing. It must all be waste of time, for though I have read many books on mechanics, I never saw anything like it."

Here the gentle voice of Mr. Anharmonic was heard. "Let us try, let us simply experiment on the mobility of the body, and let us faithfully record what we find." In justification of this advice Mr. Anharmonic made a remark which was new to most members of the committee; he asserted that *though the constraints may be of endless variety and complexity there can be only a very limited variety in the types of possible mobility.*

It was therefore resolved to make a series of experiments with the simple object of seeing how the body could be moved. Mr. Cartesian, having a reputation for such work, was requested to undertake the inquiry and to report to the committee. Cartesian commenced operations in accordance with the well-known traditions of his craft. He erected a cumbersome apparatus which he called his three rectangular axes. He then attempted to push the body parallel to one of these axes, but it would not stir. He tried to move the body parallel to each of the other axes, but was again unsuccessful. He then attached the body to one of the axes and tried to effect a rotation around that axis. Again he failed, for the constraints were of too elaborate a type to accommodate themselves to Mr. Cartesian's crude notions.

We shall subsequently find that the movements of the body are necessarily of an exquisitely simple type, yet such was the clumsiness and the artificial character of Mr. Cartesian's machinery that he failed to perceive the simplicity. To him it appeared that the body could only move in a highly complex manner; he saw that it could accept a composite movement consisting of rotations about two or three of his axes and simultaneous translations also parallel to two or three axes. Cartesian was a very skilful calculator, and by a series of experiments even with his unsympathetic apparatus he obtained some knowledge of the subject, sufficient for purposes in which a vivid comprehension of the whole was not required. The inadequacy of Cartesian's geometry was painfully evident when he reported to the committee on the mobility of the rigid body. "I find," he said, "that the body can neither move parallel to x , nor to y , nor to z ; neither can I make it rotate around x , nor y , nor z ; but I could push it an inch parallel to x , provided that at the same time I pushed it a foot parallel to y and a yard backwards parallel to z , and that it was also turned a degree around x , half a degree the other way around y , and twenty-three minutes and nineteen seconds around z ."

"Is that all?" asks the chairman. "Oh no," replied Mr. Cartesian, "there are other proportions in which the ingredients may be combined so as to produce a possible movement," and he was proceeding to state them when Mr. Commonsense interposed. "Stop! stop!" said he, "I can make nothing of all these figures. This jargon about x , y , and z may suffice for your calculations, but it fails to convey to my mind any clear or concise notion of the movements which the body is free to make."

Many of the committee sympathized with this view of Commonsense, and they came to the conclusion that there was nothing to be extracted from poor old Cartesian and his axes. They felt that there must be some better method, and their hopes of discovering it were raised when they saw Mr. Helix volunteer his

services and advance to the rigid body. Helix brought with him no cumbersome rectangular axes, but commenced to try the mobility of the body in the simplest manner. He found it lying at rest in a position we may call A. Perceiving that it was in some ways mobile, he gave it a slight displacement to a neighbouring position, B. Contrast the procedure of Cartesian with the procedure of Helix. Cartesian tried to force the body to move along certain routes which he had arbitrarily chosen, but which the body had not chosen; in fact the body would not take any one of his routes separately, though it would take all of them together in the most embarrassing manner. But Helix had no preconceived scheme as to the nature of the movements to be expected. He simply found the body in a certain position, A, and then he coaxed the body to move, not in this particular way or in that particular way, but any way the body liked to any new position, B.

Let the constraints be what they may—let the position B lie anywhere in the close neighbourhood of A—Helix found that he could move the body from A to B by an extremely simple operation. With the aid of a skilful mechanic he prepared a screw with a suitable pitch, and adjusted this screw in a definite position. The rigid body was then attached by rigid bonds to a nut on this screw, and it was found that the movement of the body from A to B could be effected by simply turning the nut on the screw. A perfectly definite fact about the mobility of the body has thus been ascertained. It is able to twist to and fro on a certain screw.

Mr. Querulous could not see that there was any simplicity or geometrical clearness in the notion of a screwing movement; in fact he thought it was the reverse of simple. Did not the screwing movement mean a translation parallel to an axis and a rotation around that axis? Was it not better to think of the rotation and the translation separately than to jumble together two things so totally distinct into a composite notion?

But Querulous was instantly answered by One-to-One. "Lamentable, indeed," said he, "would be a divorce between the rotation and the translation. Together they form the unit of rigid movement. Nature herself has wedded them, and the fruits of their happy union are both abundant and beautiful."

The success of Helix encouraged him to proceed with the experiments, and speedily he found a second screw about which the body could also twist. He was about to continue when he was interrupted by Mr. Anharmonic, who said, "Tarry a moment, for geometry declares that a body free to twist about two screws is free to twist about a myriad of screws. These form the generators of a graceful ruled surface known as the cylindroid. There may be infinite variety in the conceivable constraints, but there can be no corresponding variety in the character of this surface. Cylindroids differ in size, they have no difference in shape. Let us then make a cylindroid of the right size, and so place it that two of its screws coincide with those you have discovered; then I promise you that the body can be twisted about every screw on the surface. In other words, if a body has two degrees of freedom the cylindroid is the natural and the perfect general method for giving an exact specification of its mobility."

A single step remained to complete the examination of the freedom of the body. Mr. Helix continued his experiments, and presently detected a third screw, about which the body can also twist in addition to those on the cylindroid. A flood of geometrical light then burst forth and illuminated the whole theory. It appeared that the body was free to twist about ranks upon ranks of screws all beautifully arranged by their pitches on a system of hyperboloids. After a brief conference with Anharmonic and One-to-One, Helix announced that sufficient experiments of this kind had now been made. By the single screw, the cylindroid, and the family of hyperboloids, every conceivable information about the mobility of the rigid body can be adequately conveyed. Let the body have any constraints, however elaborate, yet the definite geometrical conceptions just stated will be sufficient.

With perfect lucidity Mr. Helix expounded the matter to the committee. He exhibited to them an elegant fabric of screws, each with its appropriate pitch, and then he summarized his labours by saying, "About every one of these screws you can displace the body by twisting, and what is of no less importance it will not admit of any movement which is not such a twist." The committee expressed their satisfaction with this information. It was both clear and complete. Indeed, the chairman remarked

with considerable force that *a more thorough method of specifying the freedom of the body was inconceivable.*

The discovery of the mobility of the body completed the first stage of the labours of the committee, and they were ready to commence the serious dynamical work. Force was now to be used, with the view of experimenting on the behaviour of the body under its influence. Elated by their previous success the committee declared that they would not rest satisfied until they had again obtained the most perfect solution of the most general problem.

"But what is force?" said one of the committee. "Send for Mr. Cartesian," said the chairman, "we will give him another trial." Mr. Cartesian was accordingly requested to devise an engine of the most ferocious description wherewith to attack the rigid body. He was promptly ready with a scheme, the weapons being drawn from his trusty but old-fashioned armoury. He would erect three rectangular axes, he would administer a tremendous blow parallel to each of these axes, and then he would simultaneously apply to the body a forcible couple around each of them; this was the utmost he could do.

"No doubt," said the chairman, "what you propose would be highly effective, but, Mr. Cartesian, do you not think that while you still retained the perfect generality of your attack, you might simplify your specification of it? I confess that these three blows all given at once at right angles to each other, and these three couples which you propose to impart at the same time, rather confuse me. There seems a want of unity somehow. In short, Mr. Cartesian, your scheme does not create a distinct geometrical image in my mind. We gladly acknowledge its suitability for numerical calculation, and we remember its famous achievements, but it is utterly inadequate to the aspirations of this committee. We must look elsewhere."

Again Mr. Helix stepped forward. He reminded the committee of the labours of Mathematician Poinset, and then he approached the rigid body. Helix commenced by clearing away Cartesian's arbitrary scaffolding of rectangular axes. He showed how an attack of the most perfect generality could be delivered in a form that admitted of concise and elegant description. "I shall," he said, "administer a blow upon the rigid body from some unexpected direction, and at the same instant I shall apply a vigorous couple in a plane perpendicular to the line of the blow."

A happy inspiration here seized upon Mr. Anharmonic. He knew, of course, that the efficiency of a couple is measured by its moment—that is, by the product of a force and a linear magnitude. He proposed, therefore, to weld Poinset's force and couple into the single conception of a *wrench* on a screw. The force would be directed along the screw while the moment of the couple would equal the product of the force and the pitch of the screw. "A screw," he said, "is to be regarded merely as a directed straight line with an associated linear magnitude called the pitch. The screw has for us a dual aspect of much significance. No small movement of the body is conceivable which does not consist of a twist about a screw. No set of forces could be applied to the body which were not equivalent to a wrench upon a screw. Every one remembers the two celebrated rules that forces are compounded like rotations and that couples are compounded like translations. These may now be replaced by the single but far more compendious rule which asserts that wrenches and twists are to be compounded by identical laws. Would you unite geometry with generality in your dynamics? It is by screws, and screws only, that you are enabled to do so."

These ideas were rather too abstract for Cartesian, who remarked that as D'Alembert's principle provided for everything in dynamics screws could not be needed. Mr. Querulous sought to confirm him by saying that he did not see how screws helped the study either of Foucault's Pendulum or of the Precession of the Equinoxes.

Such absurd observations kindled the intellectual wrath of One-to-One, who rose and said, "In the development of the natural philosopher two epochs may be noted. At the first he becomes aware that problems exist. At the second he discovers their solution. Querulous has not yet reached the first epoch, he cannot even conceive those problems which the 'Theory of Screws' proposes to solve. I may however inform him that the 'Theory of Screws' is not a general dynamical calculus. It is the discussion of a particular class of dynamical problems which do not admit of any other enunciation except that which the theory itself provides. Let us hope that ere our labours have ended Mr. Querulous may obtain some glimmering of the subject."

The chairman happily assuaged matters. "We must pardon," he said, "the vigorous language of our friend Mr. One-to-One. His faith in geometry is boundless. In fact he is said to believe that the only real existence in the universe is anharmonic ratio. It is also his opinion that if a man travelled sufficiently far along a straight line in one direction he will ultimately arrive at the point from which he started. The committee would be glad to see Mr. Querulous making the trial."

It was obvious that screws were indispensable alike for the application of the forces and for the observation of the movements. Special measuring instruments were devised by which the positions and pitches of the various screws could be carefully ascertained. All being ready the first experiment was commenced.

A screw was chosen quite at random, and a great impulsive wrench was administered thereon. In the infinite majority of cases this would start the body into activity, and it would commence to move in the only manner possible—*i.e.* it would begin to twist about some screw. It happened, however, that this first experiment was unsuccessful; the impulsive wrench failed to operate, or at all events the body did not stir. "I told you it would not do," shouted Querulous, though he instantly subsided when One-to-One glanced at him.

Much may often be learned from an experiment which fails, and the chairman sagaciously accounted for the failure, and in doing so directed the attention of the committee to an important branch of the subject. The mishap was due, he thought, to some reaction of the constraints which had neutralized the effect of the wrench. He believed it would save time in their future investigations if these reactions could be first studied and their number and position ascertained.

To this suggestion Mr. Cartesian demurred. He urged that it would involve an endless task. "Look," he said, "at the complexity of the constraints: how the body rests on these surfaces here; how it is fastened by links to those points there; how there are a thousand-and-one ways in which reactions might originate." Mr. Commonsense and other members of the committee were not so easily deterred, and they determined to work out the subject thoroughly. At first they did not see their way clearly, and much time was spent in misdirected attempts. At length they were rewarded by a curious and unexpected discovery, which suddenly rendered the obscure reactions perfectly transparent.

A trial was being made upon a body which had only one degree of freedom; was, in fact, only able to twist about a single screw, X. Another screw, Y, was speedily found, such that a wrench thereon failed to disturb the body. It now occurred to the committee to try the effect of interchanging the relation of these screws. They accordingly arranged that the body should be left only free to twist about Y, while a wrench was applied on X. Again the body did not stir. The importance of this fact immediately arrested the attention of the more intelligent observers, for it established the following general law: If a wrench on X fails to move a body only free to twist about Y, then a wrench on Y must be unable to move a body only free to twist about X. It was determined to speak of two screws when related in this manner as *reciprocal*.

Some members of the committee did not at first realize the significance of this discovery. Their difficulty arose from the restricted character of the experiments by which the law of reciprocal screws had been suggested. They said, "You have shown us that this law is observed in the case of a body only free to twist about one screw at a time; but how does this teach anything of the general case in which the body is free to twist about whole shoals of screws?" Mr. Commonsense immediately showed that the discovery could be enunciated in a quite unobjectionable form. "The law of reciprocal screws," he said, "does not depend upon the constraints or the limitations of the freedom. It may be expressed in this way: *Two screws are reciprocal when a small twist about either can do no work against a wrench on the other.*"

This important step at once brought into view the whole geometry of the reactions. Let us suppose that the freedom of the body was such that it could twist about all the screws of a system which we shall call U. Let all the possible reactions form wrenches on the screws of another system, V. It then appeared that every screw upon U is reciprocal to every screw upon V. A body might therefore be free to twist about every screw of V and still remain in equilibrium, notwithstanding the presence of a wrench on every screw of U. A body free to

twist about all the screws of V can therefore be only partially free. Hence V must be one of those few types of screw system already discussed. It was, accordingly, found that the single screw, the cylindroid, and the set of hyperboloids completely described every conceivable reaction from the constraints just as they described every conceivable kind of freedom. The committee derived much encouragement from these discoveries; they felt that they must be following the right path, and that the bounty of Nature had already bestowed on them some earnest of the rewards they were ultimately to receive.

It was with eager anticipation that they now approached the great dynamical question. They were to see what would happen if the impulsive wrench were not neutralized by the reactions of the constraints. The body would then commence to move—that is, to twist about some screw which it would be natural to call the instantaneous screw. To trace the connexion between the impulsive screw and the corresponding instantaneous screw was the question of the hour. Before the experiments were commenced, some shrewd member remarked that the issue had not yet been presented with the necessary precision. "I understand," he said, "that when you apply a certain impulsive wrench, the body will receive a definite twist velocity about a definite screw; but the converse problem is ambiguous. Unless the body be quite free, there are myriads of impulsive screws corresponding to but one instantaneous screw." The chairman perceived the difficulty, and not in vain did he appeal to the geometrical instinct of Mr. One-to-One, who at once explained the philosophy of the matter, dissipated the fog, and disclosed a fresh beauty in the theory.

"It is quite true," said Mr. One-to-One, "that there are myriads of impulsive screws, any one of which may be regarded as the correspondent to a given instantaneous screw, but it fortunately happens that among these myriads there is always one screw so specially circumstanced that we may select it as the correspondent, and then the ambiguity will have vanished."

As several members were not endowed with the geometrical insight possessed by One-to-One, they called on him to explain how this special screw was to be identified; accordingly he proceeded:—"We have already ascertained that the constraints permit the body to be twisted about any screw of the system, U. Out of the myriads of impulsive screws corresponding to a single instantaneous screw it always happens that one, but never more than one, lies on U. This is the special screw. No matter where the impulsive wrench may lie throughout all the realms of space, it may always be exchanged for a precisely equivalent wrench lying on U. Without the sacrifice of a particle of generality, we have neatly circumscribed the problem. For one impulsive screw there is one instantaneous screw, and for one instantaneous screw there is one impulsive screw."

The experiments were accordingly resumed. An impulsive screw was chosen, and its position and its pitch were both noted. An impulsive wrench was administered, the body commenced to twist, and the instantaneous screw was ascertained by the motion of marked points. The body was brought to rest. A new impulsive screw was then taken. The experiment was again and again repeated. The results were tabulated, so that for each impulsive screw the corresponding instantaneous screw was shown.

Although these investigations were restricted to screws belonging to the system which expressed the freedom of the body, yet the committee became uneasy when they reflected that the screws of that system were still infinite in number, and that consequently they had undertaken a task of infinite extent. Unless some compendious law should be discovered, which connected the impulsive screw with the instantaneous screw, their experiments would indeed be endless. Was it likely that such a law could be found—was it even likely that such a law existed? Mr. Querulous decidedly thought not. He pointed out how the body was of the most hopelessly irregular shape and mass, and how the constraints were notoriously of the most embarrassing description. It was therefore, he thought, idle to search for any geometrical law connecting the impulsive screw and the instantaneous screw. He moved that the whole inquiry be abandoned. These sentiments seemed to be shared by other members of the committee. Even the resolution of the chairman began to quail before a task of infinite magnitude. A crisis was imminent—when Mr. Anharmonic rose.

"Mr. Chairman," he said, "Geometry is ever ready to help even the most humble inquirer into the laws of Nature, but Geometry reserves her most gracious gifts for those who interro-

gate Nature in the noblest and most comprehensive spirit. That spirit has been ours during this research, and accordingly Geometry in this our emergency places her choicest treasures at our disposal. Foremost among these is the powerful theory of homographic systems. By a few bold extensions we create a comprehensive theory of homographic screws. All the impulsive screws form one system, and all the instantaneous screws form another system, and these two systems are homographic. Once you have realized this, you will find your present difficulty cleared away. You will only have to determine a few pairs of impulsive and instantaneous screws by experiment. The number of such pairs need never be more than seven. When these have been found, the homography is completely known. The instantaneous screw corresponding to every impulsive screw will then be completely determined by geometry both pure and beautiful." To the delight and amazement of the committee, Mr. Anharmonic demonstrated the truth of his theory by the supreme test of fulfilled prediction. When the observations had provided him with a number of pairs of screws, one more than the number of degrees of freedom of the body, he was able to predict with infallible accuracy the instantaneous screw corresponding to any impulsive screw. Chaos had gone. Sweet order had come.

A few days later the chairman summoned a special meeting in order to hear from Mr. Anharmonic an account of a discovery he had just made, which he believed to be of signal importance, and which he was anxious to demonstrate by actual experiment. Accordingly the committee assembled, and the geometer proceeded as follows:—

"You are aware that two homographic ranges on the same ray possess two double points, whereof each coincides with its correspondent; more generally when each point in space, regarded as belonging to one homographic system, has its correspondent belonging to another system; then there are four cases in which a point coincides with its correspondent. These are known as the four double points, and they possess much geometrical interest. Let us now create conceptions of an analogous character suitably enlarged for our present purpose. We have discovered that the impulsive screws and the corresponding instantaneous screws form two homographic systems. There will be a certain limited number (never more than six) of double screws common to these two systems. As the double points in the homography of point systems are fruitful in geometry, so the double screws in the homography of screw systems are fruitful in dynamics."

A question for experimental inquiry could now be distinctly stated. Does a double screw possess the property that an impulsive wrench delivered thereon will make the body commence to move by twisting about the same screw? This was immediately tested. Mr. Anharmonic, guided by the indications of homography, soon pointed out the few double screws. One of these was chosen; a vigorous impulsive wrench was imparted thereon. The observations were conducted as before: the anticipated result was triumphantly verified, for the body commenced to twist about the identical screw on which the wrench was imparted. The other double screws were similarly tried, and with a like result. In each case the instantaneous screw was identical both in pitch and in position with the impulsive screw.

"But surely," said Mr. Querulous, "there is nothing wonderful in this. Who is surprised to learn that the body twists about the same screw as that on which the wrench was administered? I am sure I could find many such screws. Indeed, the real wonder is not that the impulsive screw and the instantaneous screw are ever the same, but that they are ever different."

And Mr. Querulous proceeded to illustrate his views by experiments on the rigid body. He gave the body all sorts of impulses, but, in spite of all his endeavours, the body invariably commenced to twist about some screw which was *not* the impulsive screw. "You may try till Doomsday," said Mr. Anharmonic, "you will never find any besides the few I have indicated."

It was thought convenient to assign a name to these remarkable screws, and they were accordingly designated the *principal screws of inertia*. There are, for example, six principal screws of inertia when the body is perfectly free, and two when the body is free to twist about the screws of a cylindroid. The committee regarded the discovery of the principal screws of inertia as the most remarkable result they had yet obtained.

Mr. Cartesian was very unhappy. The generality of the subject was too great for his comprehension. He had an

invincible attachment to the x, y, z , which he regarded as the *ne plus ultra* of dynamics. "Why will you burden the science," he sighs, "with all these additional names? Can you not express what you want without talking about cylindroids, and twists, and wrenches, and impulsive screws, and instantaneous screws, and all the rest of it?" "No," said Mr. One-to-One, "there can be no simpler way of stating the results than that natural method we have followed. You would not object to the language if your ideas of the natural phenomena had been sufficiently capacious. We are dealing with questions of perfect generality, and it would involve a sacrifice of generality were we to speak of the movement of a body except as a twist, or of a system of forces except as a wrench."

"But," said Mr. Commonsense, "can you not as a concession to our ignorance tell us something in ordinary language which will give an idea of what you mean when you talk of your 'principal screws of inertia?' Pray for once sacrifice this generality you prize so much and put the theory into some extreme shape that ordinary mortals can understand."

Mr. Anharmonic would not condescend to comply with this request, so the chairman called upon Mr. One-to-One, who somewhat ungraciously consented. "I feel," said he, "the request to be an irritating one. Extreme cases generally make bad illustrations of a general theory. That zero multiplied by infinity may be anything is surely not a felicitous exhibition of the perfections of the multiplication table. It is with reluctance that I divest the theory of its flowing geometrical habit, and present it only as a stiff conventional guy from which true grace has departed."

"Let us suppose that the rigid body, instead of being constrained as heretofore in a perfectly general manner, is subjected merely to a special type of constraint. Let it, in fact, be only free to rotate around a fixed point. The beautiful fabric of screws, which so elegantly expressed the latitude permitted to the body before, has now degenerated into a mere horde of lines all stuck through the point. Those varieties in the pitches of the screws which gave colour and richness to the fabric have also vanished, and the pencil of degenerate screws have a monotonous zero of pitch. Our general conceptions of mobility have thus been horribly mutilated and disfigured before they can be adapted to the old and respectable problem of the rotation of a rigid body about a fixed point. For the dynamics of this problem the wrenches assume an extreme and even monstrous type. Wrenches they still are, as wrenches they ever must be, but they are wrenches on screws of infinite pitch; they have ceased to possess definite screws as homes of their own. We often call them couples."

"Yet so comprehensive is the doctrine of the principal screws of inertia that even to this extreme problem the theory may be applied. The principal screws of inertia reduce in this special case to the three principal axes drawn through the point. In fact, we see that the famous property of the principal axes of a rigid body is merely a very special application of the general theory of the principal screws of inertia. Everyone who has a particle of mathematical taste lingers with fondness over the theory of the principal axes. Learn, therefore," says One-to-One in conclusion, "how great must be the beauty of a doctrine which comprehends the theory of principal axes as the merest outlying detail."

Another definite stage in the labours of the committee had now been reached, and accordingly the chairman summarized the results. He said that a geometrical solution had been obtained of every conceivable problem as to the effect of impulse on a rigid body. The impulsive screws and the corresponding instantaneous screws formed two homographic systems. Each screw in one system determined its corresponding screw in the other system, just as in two anharmonic ranges each point in one determines its correspondent in the other. The double screws of the two homographic systems are the principal screws of inertia. He remarked, in conclusion, that the geometrical theory of homography and the present dynamical theory mutually illustrated and interpreted each other.

There was still one more problem which had to be brought into shape by geometry, and submitted to the test of experiment.

The body is lying at rest though gravity and many other forces are acting upon it. These forces constitute a wrench which must lie upon a screw of the reciprocal system, inasmuch as it is neutralized by the reaction of the constraints. Let the body be displaced from its initial position by a small twist. The wrench will no longer be neutralized by the reaction of the con-

straints; accordingly when the body is released it will commence to move. So far as the present investigations are concerned these movements are small oscillations. Attention was therefore directed to these small oscillations. The usual observations were made, and Helix reported them to be of a very perplexing kind. "Surely," said the chairman, "you find the body twisting about some screw, do you not?" "Undoubtedly," said Helix; "the body can only move by twisting about some screw; but, unfortunately, this screw is not fixed, it is indeed moving about in such an embarrassing manner that I can give no intelligible account of the matter." The chairman appealed to the committee not to leave the interesting subject of small oscillations in such an unsatisfactory state. Success had hitherto guided their efforts. Let them not separate without throwing the light of geometry on this obscure subject.

Mr. Querulous here said he must be heard. He protested against further waste of time; there was nothing for them to do. Everybody knew how to investigate small oscillations; the equations were given in every book on mechanics. You had only to write down these equations, and scribble away till you got out something or other. But the more intelligent members of the committee took the same view as the chairman. They did not question the truth of the formulæ which to Querulous seemed all-sufficient, but they wished to see what geometry could do for the subject. Fortunately this view prevailed, and new experiments were commenced under the direction of Mr. Anharmonic. He first quelled the elaborate oscillations which had so puzzled the committee; he reduced the body to rest, and then introduced the subject as follows:—

"The body now lies at rest. I displace it a little, and I hold it in its new position. The wrench, which is the resultant of all the varied forces acting on the body, is no longer completely neutralized by the reactions of the constraints. Indeed, I can feel it in action. Our apparatus will enable us to measure the intensity of this wrench, and to determine the screw on which it acts."

A series of experiments was then made, in which the body was displaced by a twist about a screw, which was duly noted, while the corresponding evoked wrench was determined. The pairs of screws so related were carefully tabulated. When we remember the infinite complexity of the forces, of the constraints and of the constitution of the body, it might seem an endless task to determine the connexion between the two systems of screws. Here Mr. Anharmonic pointed out how exactly modern geometry was adapted to supply the wants of dynamics. The two screw systems were homographic, and when a number of pairs, one more than the degrees of freedom of the body, had been found, all was determined. This statement was put to the test. Again and again the body was displaced in some new fashion, but again and again did Mr. Anharmonic predict the precise wrench which would be required to maintain the body in its new position.

"But," said the chairman, "are not these purely statical results. How do they throw light on those elaborate oscillations which seem at present so inexplicable?"

"This I shall explain," said Anharmonic; "but I beg of you to give me your best attention, for I think the theory of small oscillations will be found worthy of it."

"Let us think of any screw, α , belonging to the system U , which expresses the freedom of the body. If α be an instantaneous screw, there will of course be a corresponding impulsive screw, θ , also on U . If the body be displaced from a position of equilibrium by a small twist about α , then the uncompensated forces produce a wrench, ϕ , which, without loss of generality, may also be supposed to lie on U . According as the screw α moves over U so will the two corresponding screws θ and ϕ also move over U . The system represented by α is homographic with both the systems of θ and of ϕ respectively. But two systems homographic with the same system are homographic with each other. Accordingly the θ system and the ϕ system are homographic. There will therefore be a certain number of double screws (not more than six) common to the systems θ and ϕ . Each of these double screws will of course have its correspondent in the α system, and we may call them $\alpha_1, \alpha_2, \&c.$, their number being equal to the degrees of freedom of the body. These screws are most curiously related to the small oscillations. We shall first demonstrate by experiment the remarkable property they possess."

The body was first brought to rest in its position of equilibrium. One of the special screws α having been carefully determined

both in position and in pitch, the body was displaced by a twist about this screw and was then released. As the forces were uncompensated, the body of course commenced to move, but the oscillations were of unparalleled simplicity. With the regularity of a pendulum the body twisted to and fro on this screw, just as if it were actually constrained to this motion alone. The committee were delighted to witness a vibration so graceful, and, remembering the complex nature of the ordinary oscillations, they appealed to Mr. Anharmonic for an explanation. This he gladly gave, not by means of complex formulae, but by a line of reasoning that was highly commended by Mr. Commosense, and such that even Mr. Querulous could understand.

"This pretty movement," said Mr. Anharmonic, "is due to the nature of the screw α_1 . Had I chosen any screw at random, the oscillations would, as we have seen, be of a very complex type; for the displacement will always evoke an uncompensated wrench, in consequence of which the body will commence to move by twisting about the instantaneous screw corresponding to that wrench; and of course this instantaneous screw will usually be quite different from the screw about which the displacement was made. But you will observe that α_1 has been chosen as a screw in the instantaneous system, corresponding to one of the double screws in the θ and ϕ systems. When the body is twisted about α_1 , a wrench is evoked on the double screw, but as α_1 is itself the instantaneous screw, corresponding to the double screw, the only effect of the wrench will be to make the body twist about α_1 . Thus we see that the body will twist to and fro on α_1 for ever. Finally, we can show that the most elaborate oscillations the body can possibly have may be produced by compounding the simple vibrations on these screws $\alpha_1, \alpha_2, \&c.$ "

Great enlightenment was now diffused over the committee, and even Mr. Querulous began to think there must be something in it. Cordial unanimity prevailed among the members, and it was appropriately suggested that the screws of simple vibration should be called *harmonic screws*. This view was adopted by the chairman, who said he thought he had seen a similar expression in "Thomson and Tait."

The final meeting showed that real dynamical enthusiasm had been kindled in the committee. Vistas of great mathematical theories were opened out in many directions. One member showed how the theory of screws could be applied not merely to a single rigid body but to any mechanical system whatever. He sketched a geometrical conception of what he was pleased to call a *screw-chain*, by which he said he could so bind even the most elaborate system of rigid bodies that they would be compelled to conform to the theory of screws. Nay, soaring still further into the empyrean, he showed that all the instantaneous motions of every molecule in the universe were only a twist about one screw-chain while all the forces of the universe were but a wrench upon another.

Mr. One-to-One expounded the "Ausdehnungslehre," and showed that the theory of screws was closely related to parts of Grassman's great work; while Mr. Anharmonic told how Plücker, in his celebrated "Neue Geometrie des Raumes," had advanced some distance towards the theory of screws, but still had never touched it.

The climax of mathematical eloquence was attained in the speech of Mr. Querulous, who, with new-born enthusiasm, launched into appalling speculations. He had evidently been reading his "Cayley," and had become conscious of the poverty of geometrical conception arising from our unfortunate residence in a space of an arbitrary and unsymmetrical description.

"Three dimensions," he said, "may perhaps be enough for an intelligent geometer. He may get on fairly well without a four-dimensional space, but he does most heartily remonstrate against a flat infinity. Think of infinity," he cries, "as it should be, perhaps even as it is. Talk not of your scanty straight line at infinity and your miserable pair of circular points. Boldly assert that infinity is an ample quadric, and not the mere ghost of one; and then geometry will become what geometry ought to be. Then will every twist resolve itself into a right vector and a left vector, as the genius of Clifford proved. Then will the 'theory of screws' shed away some few adhering deformities, and fully develop its shapely proportions. Then will—" But here the chairman said he feared the discussion was beginning to enter rather wide ground. For his part he was content with the results of the experiments, even though they had been conducted in the rapid old space of Euclid. He reminded them that their labours were now completed, for they had ascertained everything relating to the rigid

body which had been committed to them. He hoped they would agree with him that the inquiry had been an instructive one. They had been engaged in the study of Nature. They had approached the problems in the true philosophical spirit, and the rewards they had obtained proved that

"Nature never did betray
The heart that truly loved her."

NOTES.

AT a public meeting held on Tuesday in Newcastle, under the presidency of the Mayor, Sir B. L. Brown, it was finally decided, on the motion of the Sheriff, Alderman W. H. Stephenson, seconded by Prof. Philipson, head of the medical staff at the Royal Infirmary, that a cordial invitation should be sent to the British Association to hold their annual meeting in Newcastle in 1889. It was stated that the necessary amount to cover expenses would be £4000, and of this £1700 had been already subscribed.

THE New York meeting of the American Association for the Advancement of Science seems to have been very successful, although the attendance was not so large as had been expected. The next meeting will be held in Cleveland, O. An invitation from Toronto came just too late. The following are the officers for the next meeting:—President, J. W. Powell, of Washington; Vice-Presidents, Ormond Stone, of the University of Virginia, (Mathematics and Astronomy), A. A. Michelson, of Cleveland, (Physics), C. E. Munroe, of Newport, (Chemistry), Calvin M. Woodward, of St. Louis, (Mechanical Science), George H. Cook, of New Brunswick, (Geology and Geography), C. V. Riley, of Washington, (Biology), C. C. Abbot, of Trenton, (Anthropology), C. W. Smiley, of Washington, (Economic Science and Statistics); Permanent Secretary, F. W. Putnam, of Cambridge, (office, Salem, Mass.); General Secretary, J. C. Arthur, of La Fayette; Secretary of the Council, C. Leo Mees, of Athens; Secretaries of the Sections, C. L. Doolittle, of Bethlehem, (Mathematics and Astronomy), A. L. Kimball, of Baltimore, (Physics), William L. Dudley, of Nashville, (Chemistry), Arthur Beardsley, of Swarthmore, (Mechanical Science), George H. Williams, of Baltimore, (Geology and Geography), N. L. Britton, of New York, (Biology), Frank Baker, of Washington, (Anthropology), Charles S. Hill, of Washington, (Economic Science and Statistics).

THE twenty-fourth annual meeting of the British Pharmaceutical Conference was opened on Tuesday in the Chemical Theatre of Owens College, Manchester. There was a large attendance of members of the Association. Mr. S. R. Atkins, of Salisbury, occupied the chair, and in his presidential address invited the attention of the Conference to "a brief review of the Victorian era as it more especially affected themselves as pharmacists."

THE International Astronomical Congress met at Kiel on Monday, in the large hall of the University, under the presidency of Privy Councillor Dr. Auwers, of Berlin. There was a large assembly of astronomers, including delegates from Austria, France, Sweden and Norway, and America. The delegates were received on behalf of the Government by Herr Steinmann, Civil Governor of the province of Schleswig-Holstein, and on the part of the University by the Rector, Prof. Harsen. Dr. Auwers, in replying, thanked the Prussian Government for the interest which it had manifested in the Congress.

THE Hygienic Congress, which will meet in Vienna next month, will be attended by over 1400 delegates from all countries. The programme includes excursions to the Kahlenberg, the Semmering, Buda-Pesth, and Abbazia.

THE Academy of Aërostation of France has presented a medal to M. Mendeleeff, the astronomer, in recognition of the pluck exhibited by him at Klin on August 19, when he went up alone

in a balloon, although he had never been in one before. The Russian Ambassador in Paris has undertaken to transmit the medal to M. Mendeleieff.

THE *Ceylon Observer* of August 1 announces the death on July 31 of Mr. W. Ferguson at the age of sixty-seven. Mr. Ferguson arrived in Ceylon in December 1839, and at once entered upon the arduous duties of Surveyor to the Government, a post which he filled for many years. He finally relinquished it very much shattered in constitution from exposure to climate. He was an enthusiastic naturalist, and employed the opportunities his profession afforded him for observation with pleasure to himself and advantage to others. Botany especially profited by his knowledge and exertions. He contributed largely to Thwaites's "Enumeratio Plantarum Zeylanice," and also to other works relating to the vegetation of Ceylon; and his aid was warmly acknowledged by the various authors whom he assisted. He was of much service to the Eclipse Expedition of 1871.

THE death is announced of Dr. Vincenz Kosteletzky, formerly Professor at the University, and the Director of the Botanical Gardens, at Prague. He died on August 19 at the age of eighty-seven.

THE Council of the Institution of Civil Engineers has issued a list of subjects on which it invites original communications. For approved papers the Council has the power to award premiums, arising out of special funds bequeathed for the purpose.

THE *Times* of Tuesday printed some notes about the eclipse which had reached German papers from Siberia and various stations in the Russian eastern provinces. At Tomsk the astronomers were able to observe not only the total eclipse but the corona in a very satisfactory way. In most houses it was necessary to light candles or lamps. The eclipse began at 10.22 a.m., and ended at 11.46. The weather was very fine and the sky clear. At Krasnoyarsk, in the Government of Yeniseisk, the corona was very well photographed. At Irbit the period of absolute totality was at 8.44 a.m., and lasted 1½ minute. Prof. Stanoievich, from Belgrade, was very successful in his observations at Petrovsk; he saw and photographed the green line in the corona. Prof. Kononovich, of Odessa, was equally fortunate, obtaining photographs of the whole spectrum. At Ekaterinburg the eclipse began in a cloudless sky at 7.25 a.m., and lasted till 9.30. The temperature fell from 19° C. to 13° (about 55½° F.) at 8.37 a.m., and rose to 24° (over 75° F.) after the eclipse. At Novochoerkask the sky was cloudless, but only about a quarter of the sun's surface was obscured, the appearance presented being a reaping-hook with the handle and point uppermost. Photographic sketches were taken every five minutes. At Savidovo the sky became suddenly clouded as the moment of the eclipse approached, and the sun was not visible till noon. The actual moment of the total eclipse could only be noted by the intense darkness which suddenly spread over the whole district. Here and there a yellowish or leaden-gray tint could be distinguished in the clouds, presenting a most weird appearance; and the strangeness of the scene was heightened by the profound disquiet and fear which seemed to have taken possession of the birds and the cattle in the fields.

PROF. YOUNG has returned from Russia, and is attending the Manchester meeting of the British Association.

THERE is a chance that, although the English Technical Education Bill has been abandoned, the corresponding Scotch measure may become law. The House of Commons went into Committee on the Bill on Monday night.

ACCORDING to the Meteorological Council, the telegrams received from the Ben Nevis Observatory have been of no service whatever as aids to the issue of storm warnings from the Meteorological Office. Mr. A. Buchan, in a letter to Mr.

R. H. Scott, complains that the memorandum in which this judgment is pronounced is very misleading. "The finding of the memorandum," says Mr. Buchan, "is that the telegrams received from the Ben Nevis Observatory are absolutely useless to the Meteorological Office in issuing storm warnings. This statement is so incomplete that we do not think that in preparing the memorandum for your report the instructions have been kept in view which were sent to Mr. Omond, in accordance with your letter of December 3, 1883, a copy of which, so far as refers to this matter, is herewith sent. A copy of this letter was sent to Mr. Omond, with instructions to carry out your wishes to the best of his ability. Now, in these instructions to Mr. Omond no special mention is made of storms or storm warnings; and certainly neither the directors nor the staff at the Observatory have ever supposed that it was expected by the Meteorological Office that a telegram was to be sent for every storm that had actually broken out or appeared to be threatened. This, however, is the assumption of the memorandum. We therefore think that in these circumstances the finding of the memorandum will be misleading to those of the public who have little or no knowledge of meteorological matters, and of the nature of the information asked from Ben Nevis Observatory; and as regards others it may be considered as doubtful if the finding that weather telegrams from Ben Nevis Observatory are useless will be indorsed by them. As you are aware, the directors offered the Meteorological Office, in their letter of November 16, 1883, daily weather telegrams from both Ben Nevis Observatory and the low-level station at Fort William. This offer, however, the Meteorological Council did not see their way to accept, chiefly on the ground of the expense; but asked for telegrams whenever any very striking change of conditions or a special phenomenon of great interest was recorded. This has been done by Mr. Omond, and, so far as the directors are aware, no application has been made by the Meteorological Office for more frequent telegrams or for any other information. The directors, in view, then, of the limited nature of the information asked for, would have been surprised if any other result had been found than that stated in the memorandum."

IN Mr. Symons's "British Rainfall," which we briefly reviewed last week, it is shown that the total fall in 1886 was rather above the average, but not exceptionally so, the amount being for England and Wales 37.53 inches, for Scotland 37.31 inches, and for Ireland (four stations only) 41.61 inches. The mean of all stations was 37.59 inches, or about 7 per cent. above the average for a long series of years. Some one ought now to discuss the observations with the view of showing the probability of rainfall for each month, and also with the view of showing the seasonal rainfall for the whole period now available.

MR. EDWARD SANGER SHEPHERD has sent us a very fine photograph of lightning taken by him at Norfolk Terrace, Westbourne Grove, W., during the thunder-storm of Wednesday, August 17. While the storm lasted Mr. Shepherd exposed fifteen plates, seven of which were successful. The apparatus used was a half-plate square box camera and a portrait lens of 1½ inches aperture; the plates used were "Ilford extra rapid."

IN a letter to the *Times* of Wednesday Prof. Tyndall called attention to the very imperfect way in which lightning conductors are often set up. Some years ago a rock lighthouse on the coast of Ireland was struck and damaged by lightning; and when the facts were brought before Prof. Tyndall, as scientific adviser to the Trinity House and Board of Trade, he found that the lightning conductor had been carried down the lighthouse tower, its lower extremity being carefully embedded in a stone perforated to receive it. "If the object," says Prof. Tyndall, "had been to invite the lightning to strike the tower, a better arrangement could hardly have been adopted. I gave directions to have the

conductor immediately prolonged, and to have added to it a large terminal plate of copper, which was to be completely submerged in the sea. The obvious convenience of a chain as a prolongation of the conductor caused the authorities in Ireland to propose it, but I was obliged to veto the adoption of the chain. The contact of link with link is never perfect. I had, moreover, beside me a portion of a chain cable through which a lightning discharge had passed, the electricity in passing from link to link encountering a resistance sufficient to enable it to partially fuse the chain. The abolition of resistance is absolutely necessary in connecting a lightning conductor with the earth, and this is done by closely embedding in the earth a plate of good conducting material and of large area. The largeness of area makes atonement for the imperfect conductivity of earth. The plate, in fact, constitutes a wide door through which the electricity passes freely into the earth, its disruptive and damaging effects being thereby avoided." Prof. Tyndall understands that lightning conductors are frequently set up without any terminal plate whatever. It is said that the Bishop of Winchester's palace at Farnham is "protected" in this way. If this is true, the Bishop will be interested to hear that the "protection" is "a mockery, a delusion, and a snare."

We have received the twelfth Report of the Bradford Philosophical Society. This institution was revived two years ago, and we are glad to see from the Report that it has "a bright prospect of success." The Society is closely associated with a group of affiliated Societies in Bradford, and it has been found that this plan works well. "The joint programme of the Societies," says the Report, "is one that reflects great credit on the town, and members of the Philosophical Society would do well to avail themselves (as their membership allows) of the various lectures and excursions of the united Societies. Members of the Society may be assured of a hearty welcome." The affiliated Societies are the Historical and Antiquarian Society, the Microscopical Society, the Naturalists' Society, the Scientific Association, and the Browning Society.

LIEUT. WISSMANN, the well-known African traveller, has arrived at Mozambique. He intends to proceed to Zanzibar on his way back to Europe.

THREE large packages containing rare plants and specimens from India have been received from Calcutta by the Keeper of the Ethnological Department of the British Museum.

A SHOCK of earthquake was felt in Mexico at seven o'clock on Monday morning. The houses were shaken and the inhabitants much terrified, but no damage was done. The direction of the shock was from north to south. The shock was also felt at Chilpancingo, where two arches of an arcade in the main square were demolished, at Orizaba, Tlaltan, and Otumba.

A LARGE proportion of the salmon fry hatched out by the Severn Fishery Board at the new hatchery at Worcester this year are being reared by Mr. William Burgess in his ponds at Malvern Wells, pending their transference to the open river. It is worthy of note that the fry may be seen rising continually to the fly. Seeing that they inhabit the bottom of the river in their wild state and do not rise, this is rather remarkable. Their rate of growth does not seem to be so fast as that of other fish, although their present position is well suited to their requirements.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Miss Austin; a — Capuchin (*Cebus*) — from South America, presented by Mr. J. H. Williams; two Horned Lizards (*Phrynosoma cornutum*) from North America, presented by Mr. Maxwell Blackie; two Common Boas (*Boa*

constrictor) from Dominica, W.I., presented by Mr. A. Nicholls; Smooth Snake (*Coronella lavis*) from Hampshire, presented by Mr. Sidney G. Smith; a Lion Marmoset (*Midas rosalia*), a Peba Armadillo (*Tatusia peba*), two Blue-bearded Jays (*Cyanocorax cyanopogon*), an Ariel Toucan (*Ramphastos ariel*), three Bahama Ducks (*Dafila bahamensis*), a Laughing Gull (*Larus atricilla*) from Brazil, a Black-handed Spider Monkey (*Ateles melanochir*?) from Central America, eight Blanding's Terrapins (*Clemmys blandingi*) from Michigan, U.S.A., purchased; two Hybrid Australian Ibises (between *Ibis strictipennis* and *Ibis bernieri*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN

VARIABLE STAR IN THE RING NEBULA IN LYRA.—Herr Spitaler draws attention in the *Astronomische Nachrichten*, No. 2800, to the apparent variability of the small star near the centre of this well-known nebula. He had made himself pretty well acquainted with the nebula in September 1885, when he had sketched it, but was induced to examine it again last autumn from the note on the "ring-formed nucleus" discovered by means of photography, which Herr E. von Gothard had published in the *Astronomische Nachrichten*, No. 2749. The interior of the ring nebula appeared with a low power to be covered with a faint curtain of light, which a high power showed to be of varying intensity, so that the interior had a faint flocculent appearance; a bright speck of light was also easily recognized midway between the centre of the nebula and the inner edge of the ring on the south-west side. In the eastern portion three faint stars were seen several times, but a fourth star seen by Prof. Vogel, and shown on the photographs of the Bros. Henry, could not be made out. But on July 25 of the present year, during the visit of Prof. Young to the Vienna Observatory, on the telescope being again turned to the nebula a small star was seen at the first glance a very little north-west of the centre, just as it is shown in the Gothard photograph, but a little fainter. The following night it was seen again, but not so distinctly. The star would therefore appear to be variable, and well worth watching. The evidence of Herr von Gothard's photograph, which shows it, whilst a faint star in the neighbourhood is not represented, seems to indicate that it is particularly rich in actinic light.

NEW VARIABLE STAR.—Mr. Espin announces in Circular No. 17 of the Wolsingham Observatory that the star Birmingham 541 is variable from $6.6 \pm$ to $8.0 \pm$. The star's place for 1887 is R.A. 20h. 9m. 17s; Decl. $33^\circ 22' 0''$ N.

DISCOVERY OF A COMET.—Mr. W. R. Brooks, Red House Observatory, Phelps, New York, discovered a comet on August 24, 20h. 53m. G.M.T. Place of the comet, R.A. 8h. 33m., Decl. $29^\circ 0'$ N. It seems probable that this object is the expected comet of Olbers.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 SEPTEMBER 4-10.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 4

Sun rises, 5h. 19m.; souths, 11h. 58m. 58.3s.; sets, 18h. 39m.; decl. on meridian, $7^\circ 13'$ N.; Sidereal Time at Sunset, 17h. 33m.

Moon (at Last Quarter Sept. 10, 15h.) rises, 19h. 25m.*; souths, 1h. 9m.; sets, 7h. 4m.; decl. on meridian, $3^\circ 39'$ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian.
Mercury ...	4 38 ...	11 39 ...	18 40 ...	$11^\circ 8'$ N.
Venus ...	8 0 ...	13 17 ...	18 34 ...	$9^\circ 8'$ S.
Mars ...	1 46 ...	9 39 ...	17 31 ...	$19^\circ 59'$ N.
Jupiter ...	10 7 ...	15 11 ...	20 15 ...	$11^\circ 35'$ S.
Saturn ...	1 31 ...	9 24 ...	17 17 ...	$19^\circ 57'$ S.

* Indicates that the rising is that of the preceding evening.

Sept. 10 ... 18 ... Mercury in superior conjunction with the Sun.

Variable Stars.

Star.	R.A.	Decl.		h.	m.
U Cephei ...	0 52.3	81 16 N.	Sept.	5, 19	6 m
α Tauri ...	3 54.4	12 10 N.	"	7, 4	14 m
V Geminorum ...	7 16.8	13 19 N.	"	9,	M
R Leonis Minoris.	9 38.8	35 2 N.	"	10,	M
δ Libræ ...	14 54.9	8 4 S.	"	5, 4	31 m
U Coronæ ...	15 13.6	32 4 N.	"	9, 20	14 m
U Ophiuchi ...	17 10.8	1 20 N.	"	5, 4	52 m
		and at intervals of 20 8			
X Sagittarii ...	17 40.5	27 47 S.	Sept.	7, 22	0 m
W Sagittarii ...	17 57.8	29 35 S.	"	9, 0	0 m
U Sagittarii ...	18 25.2	19 12 S.	"	10, 5	0 m
β Lyræ ...	18 45.9	33 14 N.	"	9, 0	0 m
η Aquilæ ...	19 46.7	0 43 N.	"	7, 0	0 m
δ Cephei ...	22 25.0	57 50 N.	"	5, 2	0 m

M signifies maximum; m minimum.

Meteor-Showers.

	R.A.	Decl.	
Near φ Tauri ...	62	37 N.	Swift; streaks.
" 15 Orionis ...	74	14 N.	Swift; streaks.
" α Andromedæ ...	355	39 N.	Very swift.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 22.—M. Janssen in the chair.—On the eclipse of August 19, by M. J. Janssen. The reports received from the various stations in European Russia and Prussia are generally described as unfavourable, owing to the clouded state of the weather at the critical time. A telegram, however, from M. Staniewicz states that at the Petrovsk station it was clear enough to take some photographs and to make a few observations. Much regret is expressed that so few observers could be induced to visit the Siberian stations, where much more successful studies might have been made.—On the cooling of the terrestrial crust, by M. Faye. This is a protest against the Rev. Ch. Braun, who, in his recent work on "Cosmogony from the Stand-point of Christian Science," adopts without acknowledgment the author's fundamental theory that the chilling process goes on more rapidly and more deeply under the seas than under the continents. M. Faye complains that M. Braun refers to him by name when criticizing his views, but omits to do so when adopting and reproducing them.—Solution of a problem, by M. J. Bertrand. Supposing a scrutiny of the ballot for two candidates, A and B, the number of voters being μ ; A, the successful candidate, obtaining m and B $\mu - m$ votes, what is the probability that during the scrutiny the number of votes for A will throughout exceed those of his rival? A rigorously algebraic solution is given of this problem, and it is added that the result may perhaps be shown in a more direct way. Thus, if the number of voters be sixty, the successful candidate must have obtained forty-five votes in order that the probability of keeping the majority throughout the scrutiny be equal to $\frac{1}{2}$.—Remarks accompanying the presentation of a memoir on the means of avoiding collisions at sea, by M. Moise Lion. The author considered that optical signals of great intensity could alone present sufficient guarantees of penetration in foggy weather. On board ships in motion the warning signal should consist of an electric focus projecting its light obliquely to the horizon and revolving round a vertical axis. He insists on the great advantage of imparting to the light an oscillatory motion in order to increase its luminosity.—On the partial lunar eclipse partly visible at Orgères (Eure-et-Loire) on August 3, by M. Edm. Lescarbault. The shadow cast on the upper left part of the moon was almost black; but to the left, and especially to the right, there were noticed two curvilinear triangles of 2'5 to 3'5 length at base, where the shadow was ruddier than a very deep maroon. The triangle to the left was even darker than that to the right, while both were connected by a thin streak of the same colour, but deeper to the south of the moon. The inner edges of these maroon surfaces blended insensibly in the black shadow, and within them could be very faintly distinguished a few cirques, which could not be otherwise accurately determined. On the disk the shadow was edged with a grayish straw-coloured band, two and a half or three times as broad as

Tycho, the common edge of this band and of the shadow being somewhat sharply traced.—On the coefficient of self-induction of two bobbins combined in quantity, by MM. G. Maneuvrier and P. Ledeboer. In a previous paper the authors dealt with the problem whether from the stand-point of self-induction it was possible to compensate two bobbins combined in quantity by a single bobbin, and consequently whether it might be possible to assign to such a system a determined coefficient of self-induction in the strict sense of the term. A fresh series of experiments are here described which have been carried out for the purpose of determining how far the results already obtained may be approximately verified for the most general case. These experiments lead to the conclusion that for the general case the system of two bobbins cannot be compensated by a single bobbin, and consequently that such a system has no coefficient of self-induction properly so called.—On the compressibility of some solutions of gas, by M. F. Isambert. From the experiments here described the author infers that a simple solution of gas changes very little the coefficient of compressibility of the solvent; further that the solution of ammoniac gas in water behaves in the same way as that of a true chemical compound.—On the titanates of zinc, and more particularly on a trititanate, by M. Lucien Lévy. Metallic titanates are obtained either by the action of the metallic oxide on the titanate acid in the presence of the chloride or the fluoride, or else by the action of a mixture of the metallic sulphate and an alkaline sulphate on the same acid. Applied to the production of the titanates of zinc these two processes have yielded different results. The first, which is here more specially dealt with, leads in general to a trititanate. The second, on the contrary, furnishes several salts according to the proportions employed.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Science and Art Schools and Classes Directory, 1887 (Eyre and Spottiswoode).—Calendar of Durham College of Science, Newcastle-on-Tyne, 1887-88 (Newcastle-on-Tyne).—Insects Noxious to Agriculture and Plants in New Zealand: The Scale Insects: W. M. Maskell (Wellington).—Electrical Distribution by Alternating Currents and Transformers: R. Kennedy (H. Alabaster and Co.).—Proceedings of the Liverpool Naturalists' Field Club, 1886-87 (Liverpool).—Economic Forestry: Prof. Bougen.—Transactions and Proceedings of the New Zealand Institute, 1886, vol. xix. (Wellington).—Quarterly Journal of the Geological Society, vol. xliii. Part 3, No. 171 (Longmans).—Journal of Physiology, vol. viii. Nos. 3 and 4 (Cambridge).—Annalen der Physik und Chemie, 1887, No. 9 (Barth, Leipzig).—Quarterly Journal of Microscopical Science, August (Churchill).—The Asclepiad, No. 15, vol. iv.: Dr. B. W. Richardson (Longmans).

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